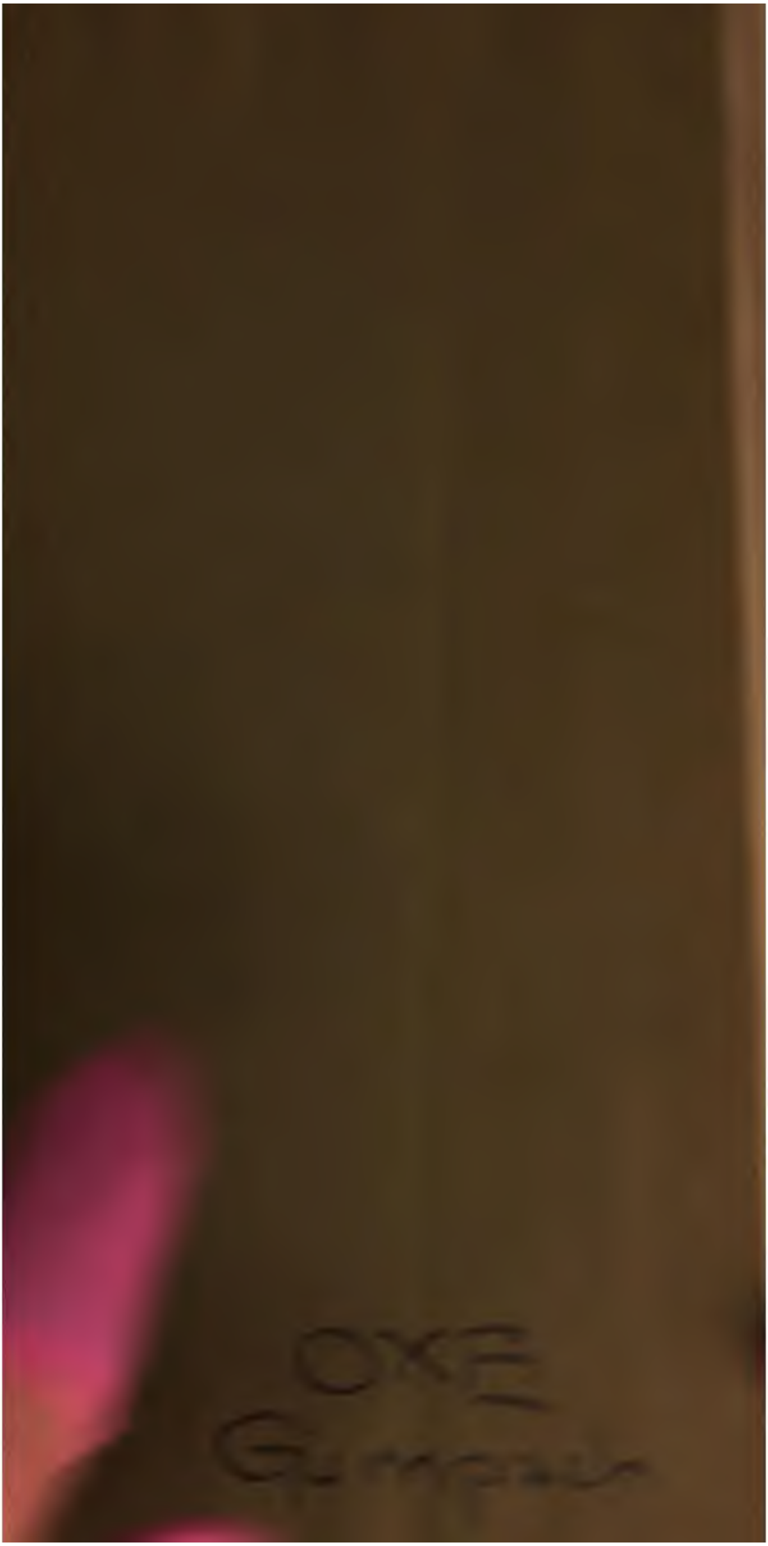

OXE

THE TRUE FIGURE AND DIMENSIONS OF THE EARTH,

NEWLY DETERMINED FROM THE RESULTS OF GEODETIC MEASUREMENTS AND PENDULUM OBSERVATIONS; COMPARED WITH THE CORRESPONDING THEORETICAL ELEMENTS, FOR THE FIRST TIME DEDUCED UPON PURELY GEOMETRICAL PRINCIPLES; AND CONSIDERED BOTH WITH REFERENCE TO THE PROGRESS OF SCIENTIFIC TRUTH, AND AS BEARING UPON THE PRACTICAL INTERESTS OF BRITISH COMMERCE AND NAVIGATION;

IN

A LETTER,

ADDRESSED TO

GEORGE BIDDELL AIRY, ESQ., M.A.
ASTRONOMER ROYAL.

BY

JOHANNES VON GUMPACH.

"Une théorie est une souris: elle était passée par neuf trous;—un dixième l'arrête."—VOLTAIRE.

SECOND EDITION, ENTIRELY RECAST,
WITH THIRTY ILLUSTRATIVE DIAGRAMS.

LONDON:
ROBERT HARDWICKE, 192, PICCADILLY.
1862.

PREFACE.

During the last century and a half, the problem of the true figure and dimensions of the Earth has been, and continues to be, one of the most prominent, as well as one of the most important, objects of scientific inquiry and practical investigation. While the results of the latter, at a vast expenditure of labour and money, have furnished us with a commensurate amount of the most accurate empirical knowledge, the efforts of the former have thus far proved completely abortive.

In the following pages, the first geometrical, and, therefore, final solution of this problem, reversing most of the ideas, which we have been accustomed to associate with it, is submitted to the severest scrutiny of the learned, in a manner intelligible to every one. "Why I should have succeeded wherein so many of the most eminent men, from the time of Sir Isaac Newton to the present day, have failed?" The reason is a very simple one. Instead of applying myself, as they have done, to the task of bending the successively collected geodetical facts,—that is to say, the Earth itself,—to a preconceived theory, solely resting on Sir Isaac Newton's erring imagination, I have adopted the more legitimate method of geometrically deducing a theory of the Earth's figure from the given geodetical facts, hitherto collected. "How I dare to oppose my individual judgment, in a matter like this, to the conclusions, arrived at, and upheld by, the whole scientific and intellectual world?" Let the reader remember, that Galileo and Copernic did the same, and were in the right. I can but, pointing to the *facts* of the case, and to *the terrestrial equator itself*, reply in the words of the apostle: Καίσαρα ἐπικαλοῦμαι.

A scientific question of extreme interest and importance in itself, and the practical bearing of which upon commerce and navigation involves the loss at sea of millions' worth of property and of thousands of human lives, needs no further title to commend it to the earnest attention and solicitude of the British Government * and the British public.

* The question,—though a point of comparatively minor consideration,—bears as well upon the National Survey, based on the principal Triangulation of the United Kingdom, which was commenced in 1783, under General Roy, and under the successive direction of Colonel Williams, General Mudge, General Colby, Colonel Hall, and

The first edition of this Letter, addressed to Mr. Airy in his official capacity as Astronomer Royal of England, appeared in the shape of a

Colonel James, has only lately been finished. The triangulation itself ranks second to none among similar national undertakings, and reflects the highest credit, both upon the country and upon the eminent men, who have been engaged in its execution. In the application of its results, however, an "Account" of which, published by order of the Master-General and Board of Ordnance, has been rendered by Captain Clarke under the direction of Colonel James (London, 1858, pp. 782, in 4to), a serious error is committed, inasmuch as, contrary to every sound principle, the facts resulting from the triangulation, and to establish which geometrically was the sole business of the survey, have been modified by mixing them up with the *theory* of the Earth's figure, and by applying, in the computation of distances, Mr. Airy's elements ("Account," pp. 283, 674, 675). The consequence is, that, as will be seen hereafter, all those distances are erroneous; and not only have they to be recalculated, but a large portion of the work itself, also, will have to be reprinted.

How far the effects of a special error of not less than ten thousand feet (nearly two miles), which I have discovered in the table containing the "distances of parallels," as finally adopted ("Account," p. 732), and of a second error, probably of a similar origin, of one thousand (or nearly one thousand) feet, may extend through the work, I have had no leisure to examine. It is a subject, which will have to engage the attention of the superintendent of Her Majesty's Ordnance Survey.

It would appear, to use Mr. Airy's own words, that "the practice to employ, in the geodetic calculations made in the Ordnance Map Office, the elements which he (the Astronomer Royal) obtained by an investigation published in the article 'Figure of the Earth' in the 'Encyclopædia Metropolitana,' was first introduced under Colonel Colby's direction."—*Airy, Determination of the Longitude of Valentia, in the Greenwich Astronomical Observations for* 1854. *Appendix*, p. ccxxviii.)

pamphlet, to which, excluding, as it did, all details of discussion, a very limited circulation was given for reasons, connected with the following correspondence between the Astronomer Royal and myself:—

"GUERNSEY, *October* 10, 1861.

"SIR,—I have the honor of transmitting to you, by this post, a printed Letter, which I am about to address to you, publicly, on the problem of the true figure of the Earth.

"In respectfully calling your attention to that Letter, and its important results—important both in a national and a scientific point of view,—I conceive that they warrant the request on my part, that you will be pleased to give to them your early consideration; and to inform me whether it is your intention to recommend to Her Majesty's Government the scientific expedition suggested by me, or to leave it to myself to take such steps in the matter as I may deem advisable.

"I have the honor to be, with profound respect, &c."

"ROYAL OBSERVATORY, GREENWICH,
"*October* 12, 1861.

"SIR,—I have had the honor this day of receiving your letter of the 10th instant, and also the printed pamphlet to which it refers, entitled (follows title at length).

"I have perused the pamphlet sufficiently to acquire a general knowledge of the character of its reasoning, and the practical step to which it is supposed to lead.

"And in reply to the suggestion on page 54, paragraph 3, I have to state, that I do not see occasion to urge upon Her Majesty's Government the propriety of an expedition to proceed to the equator for the purpose of ascertaining by actual measurement the linear extent of the Earth's equatorial circumference (page 10); with the views and for the reasons which you have detailed.

"I should be glad to have an equatorial arc measured,* as an additional contribution to our knowledge of the Earth's figure, and as possibly modifying by an insignificant quantity the numerical value of one of the elements.

"But I do not think that there is the slightest reason for doing this in respect of the considerations explained in the pamphlet cited above.

"I am, &c."

"GUERNSEY, *October* 14, 1861.

"SIR,—I have had the honor of receiving, in answer to my communication of the 10th instant, your letter of the 12th, by which you inform me, that you see no occasion to recommend to Her Majesty's Government the scientific expedition to the equator, suggested in my printed letter to you; that you should be glad to have an equatorial arc measured; but that you do not think there is the slightest

* It will be remarked, that this apparent desire, on Mr. Airy's, part, to have an arc of the equator measured, is evidently but intended to serve as a cloak for his determination to resist the proposed expedition. For, however lavish of public money the Astronomer Royal may be, his actually entertaining the idea of, or planning, a costly expedition with a view to the bare possibility of its leading to some insignificant result, would be a kind of practical joke, of the serious contemplation of which I willingly acquit Mr. Airy. His object is as manifest as is his self-contradiction, rendered more preminent still by the circumstance, that the asserted fact of the *elliptical* form of the terrestrial equator analytically deduced, a year or two ago, by General von Schubert, and which will be noticed hereafter, was by the Astronomer Royal himself pronounced to "merit the most careful attention of geodetists."

reason for doing this in respect of the considerations, explained in my pamphlet. And the grounds, upon which you express this opinion, are, that 'you have perused the pamphlet sufficiently to acquire a general knowledge of the character of its reasoning, and the practical step to which it is supposed to lead.'

"Permit me to observe, in reply, that a question, involving not only the progress of truth and of astronomical science, but, moreover, the preservation of millions' worth of national property and of the lives of thousands of our fellow-men, appears to me a proper subject, not for a general and fugitive inspection, but for a fair, a conscientious, a scrupulous examination.

"I therefore desire to offer you the opportunity of a more mature re-consideration of the opinion, at which you have rrived after a first cursory survey of my pamphlet, and ıce more to call your attention to the *conclusive* nature í the proofs and arguments, adduced in support of my propositions.

"As this is a matter of deep national concern, you will willingly consent, I doubt not, to my giving publicity to the present correspondence, on the receipt of your answer, which request at your early convenience.

"I have the honor to be, &c."

"ROYAL OBSERVATORY, GREENWICH,
"*October* 21, 1861.

"SIR,—I have to acknowledge receipt of your letter of the 14th instant, in continuation of correspondence on the Figure of the Earth.

"In reply I have to state, that I have no alteration to make in my letter of the 12th instant, and nothing to add.

"I am, &c."

"GUERNSEY, *October* 28, 1861.

"SIR,—I have the honor of acknowledging the receipt of your letter, dated the 21st instant, informing me that you 'have no alteration to make in your letter of the 12th, and nothing to add;' which leaves me to infer, that you have not thought it necessary to give to the logical and geometrical reasoning of my pamphlet on the true figure of the Earth that more mature consideration, which I ventured, a second time, to claim for results, involving to so vast an extent the interests of British commerce and navigation.

"I had reason to believe, I remarked in my pamphlet, that naked truth, in opposition to the established system of theoretical astronomy, would stand but a poor chance of a hearing. But when it has in its train, I added, the wreck of colossal national wealth, and the corpses of thousands of our fellow-beings, hurried into eternity by the abstract idea of universal gravitation, its voice is certain to make itself heard, sure to command the attention even of the Astronomer Royal.

"You have, in a signal manner, verified the former part of that statement; to verify the latter, now rests with Her Majesty's Government, the British press, and the people of England.

"I have the honor to be, &c."

It is not my wish to offer one word of further comment, either upon the position assumed by the Astronomer Royal in reference to the important question at issue, or upon the tone and spirit of his letters. The true aim, Mr. Airy has in view, is sufficiently apparent to every one, who sees below the surface, and knows something of what is passing in the astronomical world.

That the amount of plain and overwhelming evidence, both negative and positive, which is here brought to bear upon the problem discussed, should be fairly weighed and scrupulously examined, is all I desire, and need desire. The evidence is such that, in a rational and well-constituted mind, powerful enough to divest itself of its preconceived notions, it can lead to but one conclusion.

As to the scientific expedition to the equator, suggested by me for the purpose of settling, by direct measurement, the question of the Earth's equatorial extent—if question it may still be called—it is, in my opinion, imperatively called for, not with reference to any theoretical considerations, or doubts as to the general correctness of the measurement, effected in 1735 by Messrs. Bouguer and La Condamine, but on account of the vast national interests, involved in a direct verification, conducted with all the resources of modern science, of the former results.

For this reason, the new expedition will be carried into effect,—for, "unless I greatly mistake the temper of the people of England, they will not suffer the practical solution of a question of this nature and magnitude to remain long in abeyance,"—and will be carried into effect on the grounds, established in the present treatise, notwithstanding the factious opposition, offered by Mr. Airy. There are limits, beyond which the Astronomer Royal may not extend such an opposition, without disgracing the astronomical science of this country in the eyes of Europe, without rendering himself responsible to the Government, whose servant he is, and to the nation, whose servants the Government are.

JOHANNES VON GUMPACH.

GUERNSEY, *July*, 1862.

THE TRUE FIGURE OF THE EARTH.

To GEORGE BIDDELL AIRY, ESQ., M.A.,
ASTRONOMER ROYAL.

SIR,

"THE determination of the exact figure of the Earth," Mr. Biot remarks,* "has, for the last century and a half, been one of the constant aims of the labours of the French Academy of Sciences. From the time of the first measure of a degree by Picard, which enabled Newton to establish the law of universal gravitation, the highest efforts of astronomy and analysis have been directed to the consolidation of all the elements of that great phenomenon, and to the development of all the consequences, which they allow us to draw, not only as to the figure, but also as to the interior condition, of the terrestrial spheroid."

What are the results to which these protracted efforts of science have led? General von Schubert will tell us.

"The geodetic operations," that eminent geodetist

* "Traité Elém. d'Astronomie Physique," 3e. éd., tome ii., Paris, 1844, p. 460.

acknowledges before the Russian Academy of Sciences,* "carried out, during the last century and a half, for the purpose of determining the figure and the dimensions of the Earth, have up to this time led to no satisfactory results. Having been performed by the most eminent astronomers, with the most perfect instruments,—in short, with all the resources of modern science,—it would seem that they ought to have led to a final solution of this most interesting problem. Such, however, is by no means the case. Every new measure of a meridian arc has but added, and adds, to the existing doubts and want of concordance, nay, to the positive contradictions, which the various operations exhibit as compared with one another. . . . . On carefully examining the results thus far obtained, it has appeared to me that their differences arise not so much from the measurements themselves as from the method, by which astronomers have drawn their deductions from them. It has occurred to me, therefore, that, by adopting a different process, I might at last succeed in bringing about a satisfactory solution of the problem."

The analytical deductions, arrived at by General von Schubert, instead of solving the problem of the

* "Essai d'une Détermination de la Véritable Figure de la Terre" (Memoirs of the Imperial Academy of Sciences of St. Petersburgh), St. Pétersbourg, 1859, 4to. pp. 1–2.

Earth's figure, only tend to its further complication. I shall revert to them hereafter. In the meantime, the importance of his attempt has been fully admitted and appreciated by astronomers. Dr. Maedler speaks of it in terms of the highest praise,* and in your own opinion, it "merits the most careful attention of geodetists."†

The remarkable circumstance to which I would thus, in the first place, direct attention, is that in the middle of the nineteenth century, and at a time when astronomy and analysis celebrate their most brilliant triumphs, *the ground itself, on which the truth of all their practical observations and theoretical deductions mainly rests*, continues a subject of doubt and perplexity, as much as ever it was in the almost forgotten days of Sir Isaac Newton. After one hundred and fifty years of unceasing efforts, astronomy has yet to discover whether the terrestrial equator forms an ellipse or a circle; after a century and a half of unsuccessful calculation, analysis is still seen toiling to invent *empirical* formulas, for the purpose of establishing a tolerable accordance between the geodetic measurements of to-day with those of yesterday.

* "Wochenschrift für Astronomie" for 1859, p. 404.

† "Monthly Notices of the Royal Astronomical Society," vol. xx. (1859-60), p. 107.

It is this circumstance, which has induced me to submit the problem of the true figure of the Earth * to a renewed and an independent investigation; the results of which, as important as they were unexpected, place me in direct opposition with the principle of universal gravitation and the entire system of modern astronomy.

I would, therefore, invite your attention to the train of logical and geometrical reasoning, which has led me to the unavoidable conclusion that *the Earth, instead of being flattened, is elongated at the poles.* My proofs and arguments, resting on the results of geodetic measurements, astronomical observations, and universally admitted principles of geometry, are plain, simple, and intelligible. I desire to submit them to you with all the deference due to your position as Astronomer Royal, as a mathematician of note, and as the author of a special treatise on the subject of my communication.† On the other

* To prevent misconception on the part of the general reader, I may be permitted to remark that, in astronomically speaking of the true figure of the Earth, the local depressions and elevations of its surface below and above the level of the sea, are supposed to have no existence. The term "true figure" is to be so understood as though the great oceans, in a state of perfect repose and equilibrium, extended over the entire surface of our globe.

† "Figure of the Earth" in the Encyclop. Metrop., vol. v., pp. 165-240 (1830).

hand, I shall have to use that freedom of expression and unreservedness of argument, which the importance of the problem and the vast interests involved in it at once require and entitle me to claim.

I.

Astronomy teaches that the true figure of the Earth, slightly deviating from the shape of a perfect globe, is that of an *oblate* spheroid of revolution, such as is represented in the diagram, Fig. 1; *i.e.*, that the polar diameter $p\,p'$ of the Earth, the circular form of its equator and the symmetry of its proportions being assumed, is shorter than its equatorial diameter $e\,e'$ by about the $\frac{1}{299}$th part of the latter; in other words, that, to use a familiar expression, the Earth, being of the shape of an orange, is *flattened* or depressed at the poles.

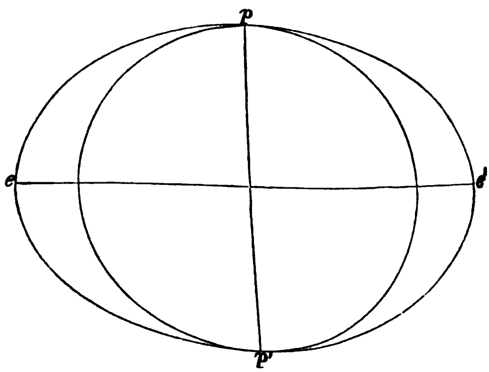


FIG. 1.

On the contrary, I shall adduce conclusive proofs that the true figure of the Earth, deviating from the perfectly globular shape to the threefold extent of what it is now supposed to do, is that of a *prolate* spheroid of revolution, such as is represented in the diagram Fig. 2; *i.e.*, that the polar diameter $p\,p'$ of the Earth, the circular form of its equator and the symmetry of its proportions being assumed, is longer than its equatorial diameter $e\,e'$ by the $\frac{1}{95}$th part of the latter; in other words, that the Earth, being of the shape of a lemon, is *elongated* at the poles.

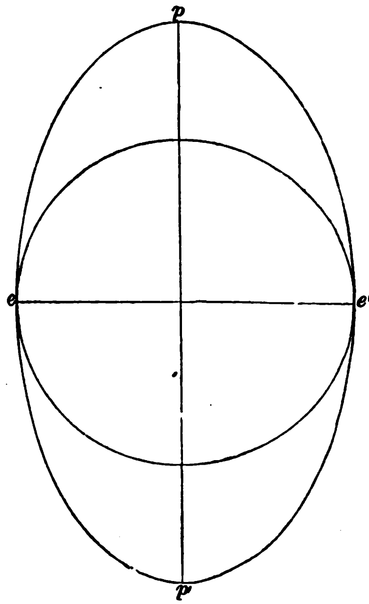


FIG. 2.

Again, astronomy teaches the linear dimensions of the Earth to be as follows* :—

	According to	
	Bessel.†	*yourself*‡
Equatorial diameter.............	7925·604	7925·648 miles.
Polar diameter.................	7899·114	7899·170 ,,
Polar compression	26·471	26·478 ,,

* Sir John Herschel, "Outlines of Astronomy," 5th edition, p. 139.

† "Astron. Nachrichten." Nos. 333, 335, 438. (1841.)

‡ "Figure of the Earth," p. 219.

and it is maintained that the error of the numerical value, here assigned to the equatorial diameter of the Earth, cannot exceed two-thirds of a mile; * and, consequently, that the circumference of the terrestrial equator, taken = 7925·626 × π = 24899·044 miles, cannot deviate from the truth by more than two miles.

On the contrary, I shall prove the linear dimensions of the Earth to be:—

		Instead of	*Difference.*	
Equatorial diameter	7872·564	7925·626	— 53·062	miles.
" circumference	24732·392	24899·044	— 166·652	"
Polar diameter	7955·433	7899·142	+ 56·291	"
		Polar compression.		
" elongation	82·869	26·475	+ 109·344	"

Hence, modern astronomy, in the computation of the Earth's polar diameter and its equatorial circumference, is at fault to the somewhat startling extent, in the former case, of one hundred and nine miles; in the latter, of one hundred and sixty-six miles.

In support of the correctness of their theoretical numbers, astronomers point to the results of actual geodetic measurements and pendulum-observations, carried out in various parts of the globe. So do I. But our modes of proceeding differ widely. Astronomers set out by moulding a fictitious Earth into a purely theoretical shape, and then, by means of the

* Sir John Herschel, "Outlines," p. 134.

vague, accommodating formulas of analysis,* adapt, or rather endeavour to adapt, the Earth's real proportions, as they result from actual measurement and observation, to that imagined form. From the results of actual measurement and observation I deduce a theory of the Earth's figure, and then compare the dimensions of our globe, computed on that theory by the simplest formulas of geometry, with its real dimensions.

The extreme importance of the question, thus raised, is obvious.

As bearing upon science, it involves the destruction f the entire system of modern theoretical and hysical astronomy.

As bearing upon navigation and commerce, it involves the preservation of millions' worth of property, and thousands of human lives.

* "I cannot but believe," the late Mr. Nichol, Professor of Astronomy in the University of Glasgow, remarks, "and my belief is founded on a reflection on *the remarkably loose, tentative, and artificial condition of our higher calculus*—that some discovery here awaits us—some grasping of a new and broad principle as the basis of a new analytic art—which shall again effect in the region of science a reformation equivalent to that which ranks among the chief of the honours of Des Cartes: and indeed it will be only then that our growing and extending apparatus for exploring the contents of the skies will reach its efficiency, as a means of revealing the character of their motions—the comprehensiveness and simplicity of their laws."—Nichol, "The Planet Neptune," p. 67.

During those hundred and fifty years, which have elapsed since the time of Sir Isaac Newton, there have perished at sea, *solely in consequence of his erroneous theory*, and at a very moderate computation, some ten thousand human beings—the majority of them British sailors;—besides property, worth from five-and-twenty to thirty millions pounds sterling.

At the present period, the *annual* losses at sea, attributable to the same cause, amount to five hundred lives, and property of the value of a million pounds sterling.

And, calculated for a space of time, embracing the next century and a half,—assuming commerce and navigation to go on progressing at but a very moderate rate,—there will be lost at sea, property worth from two to three hundred millions pounds sterling, and the lives of no fewer than one hundred thousand of our fellow-creatures, from no other cause save an erroneous astronomical theory, *unless that theory cease to be applied to the practical purposes of navigation.*

Fortunately for the sake of humanity and the interests of science and commerce, the important question at issue admits of a solution, at once simple and incontrovertible. *It turns altogether upon* ONE SINGLE FACT—*the actual linear extent of the terrestrial equator.*

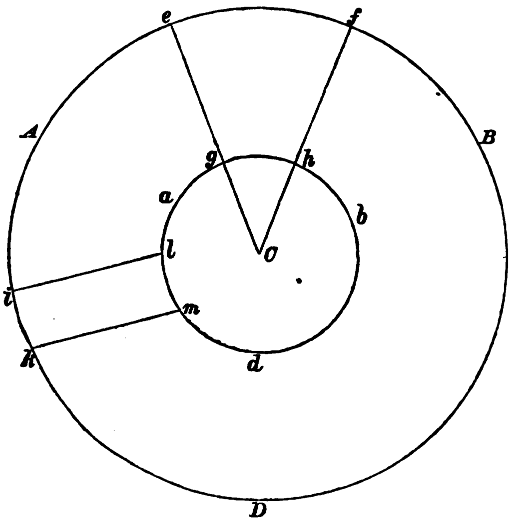
The main object of this letter, therefore, is to lay the question of the true shape of the Earth, in its leading features and its most important bearings, fairly and intelligibly before the public, with the view of directing the attention of Her Majesty's Government to the subject, and to suggest the expediency of a scientific expedition to the equator, for the purpose of ascertaining, by actual measurement, the linear extent of the Earth's equatorial circumference, and thus to set the general problem of the true figure and the true dimensions of our globe for ever at rest.

Unless I greatly mistake the temper of the people of England, they will not suffer the practical solution of a question of this nature and magnitude to remain long in abeyance.

II.

To solve the problem theoretically is all I can here attempt.

The first thing needful to be brought to the discussion, is a clear notion of the meaning of "*a degree*," both abstractedly and as applied to the determination of the Earth's true shape. Now, a "*degree*" is one of 360 equal parts, into which we, conventionally, conceive a circle to be divided by 360 right lines or *radii*, converging from 360 points in the circumference of the circle, equidistant from each other, to one common point within it, the center, equidistant from all, as well as from every other point in the circumference of the circle.



A B D—the greater circle.

a b d—the lesser circle.

C—their common center.

C e, *C f*—radii of the greater circle.

C g, *C h*—radii of the lesser circle.

e f, *g h*—arcs of one degree each.

l m—the greater curvature.

i k—the lesser curvature.

Fig. 3.

Hence, the following general rules will appear obvious from a mere inspection of the diagram, Fig. 3.

Rule i.—The linear dimensions of the circumferences of any two given circles are to each other, as are those of arcs of one degree of each circle; or as the radii of such circles are to each other (the constant numerical proportion of the circumference, linearly measured, to the radius being as 6·283185 : 1, nearly).

Rule ii.—The greater the linear dimensions of a given arc of one degree, the greater is the corresponding radius, and the greater, consequently, is the distance of every point in the given arc from the center of the circle. And *vice versâ*.

RULE III.—The greater the linear dimensions of a given arc of one degree, the less is its curvature. And *vice versâ*.

RULE IV.—The greater the curvature of a given arc of one degree, the less is the distance of every point in that arc from the center of the circle. And *vice versâ*.

RULE V.—The center of the circle being the origin of its divisions and, consequently, the origin of all angular measure: when one circle is to be compared with another, *i.e.*, when one circle is to be measured by a normal circle, divided into 360 degrees, the enter of the normal circle has to be applied to the enter of the circle to be compared with it;—the same as the origin of a linear yard-measure has to be applied to the origin of any object to be linearly measured, provided either measurement is to give a correct result. The only difference, in fact, between the yard-measure and the normal circle-measure consists in this, that the one extends from a given point of origin in one direction only, the other from a given point of origin in all directions simultaneously.

Or, supposing we were, in oppòsition to the latter rule, to apply the normal measure of a great circle, Fig. 4, to some given circle, of the exact circular

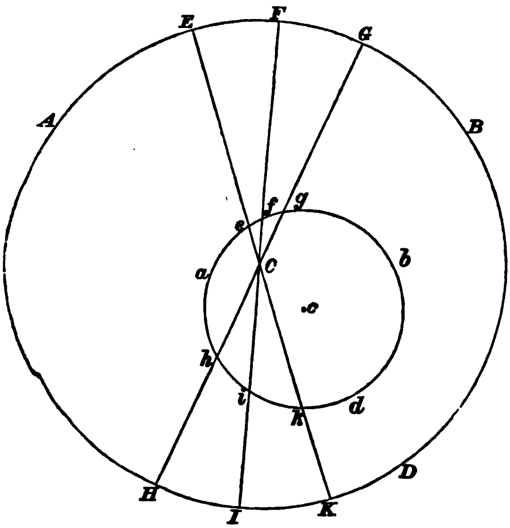
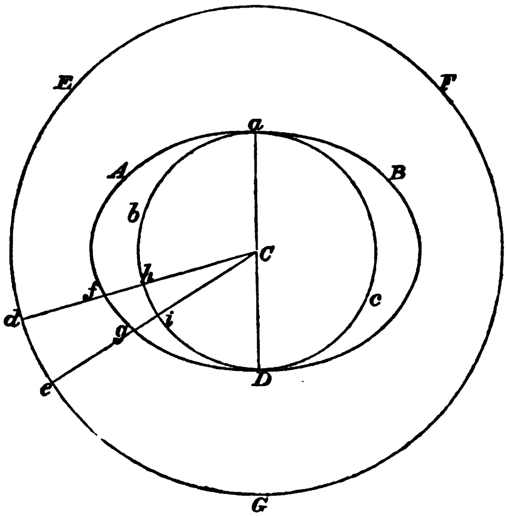


FIG. 4.

shape of which we desire to convince ourselves, in such a manner as to make the center of the normal circle fall on some point *C* in the given circle, not coinciding with its center *c*. The radii of the normal circle, supposed to contain arcs of one degree each, would in the circumference of the given circle determine the points *e*, *f*, *g*, *h*, *i*, *k*, as marking corresponding arcs *e f*, *f g*, *h i*, *i k*, of one degree each also; and on measuring the linear extent of these arcs, we should find the arcs *e f* and *f g* to measure each much less than the opposite arcs *h i* and *i k*. But if, under the impression of having measured *true* degrees on the circumference of the given circle, we were to conclude from such measurements, in accordance with Rule II., that the given circle were not truly circular, but of an egg-shape, our conclusion would be erroneous, because in direct opposition to the fact assumed. Or, if we were to apply in succession the center of the normal circle to different points within the given circle, and draw our conclusions as to its exact figure from the combined results of corresponding linear measurements, the error of our conclusion would only assume a more complicated character. And again, if we were to compute the radii of the given circle from those measurements, or its circumference from one of the arcs of a degree *e f* or *f g* and *h i* or *i k*, the results would be equally erroneous n all cases.

Nor is it different with an ellipse or any other figure of a spheroidal shape. If we wish to determine the exact proportions of any given ellipse, Fig. 5, as compared with a circle; in other words, if we wish to determine by linear measurement of degrees of a circle, on the circumference of an ellipse, to what extent and in which directions its figure deviates from a circle, we have—with the minor semi-axis of the ellipse—to describe the greatest circle within it. This circle will be the circle of comparison; and it is obvious, from what has been



A B D—the given ellipse.

E F G—the normal circle.

a b c—the circle of comparison.

C—their common center.

C D—the minor semi-axis of the ellipse = the radius of the circle of comparison.

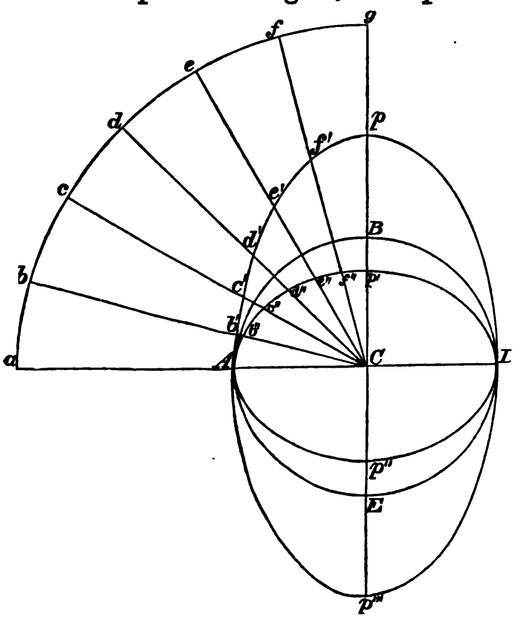
d e, *f g*, *h i* } arcs of one degree each.

FIG. 5.

explained before, that we can draw no correct conclusions from linear measurements of degrees on the circumference of the ellipse, unless we apply the center of the normal circle to the center of the circle of comparison, and consequently to the center of the ellipse itself.

But what is true of angular measurement on the circumference of spherical and elliptical planes, is true also of angular measurement on the surfaces of spheroidal bodies in general, and of the Earth in particular. For, if we conceive the Earth—assuming that it is a spheroid of revolution—to be divided through its poles and its center into two equal halves, the resulting surfaces of both halves would represent perfect planes bounded by elliptical circumferences; and since one of these planes, revolving about either its minor or its major axis, would describe the figure we assume the Earth to have, we may look upon the terrestrial globe as consisting of a great number of such planes.

That the true figure of the Earth is very nearly that of a perfect sphere, is a well ascertained fact. Let this sphere in Fig. 6, be represented by the circle.



a..g—a quadrant of the normal circle.

A B D E—the circle of comparison.

C—their common center.

a D—the equator.

a C—the equatorial radius.

$g\,p'''$—the direction of the poles.

Ca.. Gg—radii of the normal circle.

ab..fg—arcs of 15 degrees, termed of 1 degree, each.

FIG. 6.

We wish to ascertain, to what extent and in what directions the exact proportions of the Earth deviate from the spherical form. All we are supposed to be acquainted with, is the position of the equator and the direction of the poles. Then, always assuming the Earth to be a spheroid of revolution, its true shape, as compared with a circle described with its equatorial radius, is either depressed at the poles and the actual position of the latter is somewhere near p' and p''; or else it is elongated at the poles, and their actual position is somewhere near p and p'''. In the former case $A\,p'$ is the meridian arc, extending from a given point A in the equator to the north pole; in the latter it is $A\,p$. In both cases, that arc comprises 90 degrees. In both cases, the corresponding equatorial arc of 90 degrees, at right angles to the former, is $A\,B$.

Now, as we have to deduce the true figure of the Earth from the linear values of meridian degrees, obtained by actual measurement on the Earth's surface, a mere inspection of Fig. 6, will give us to this effect the following general rules, in addition to those previously laid down.

RULE VI.—The Earth is flattened or *depressed* at the poles, if the linear dimensions of any given meridian arc of 90°, extending from the equator, *i.e.*, from 0° of latitude, to the pole, *i.e.*, to 90° of latitude, are *less* (arc $A\,p'$) than the linear dimensions of an arc of 90°, as computed with the linear value of a meridian arc of one degree in 0° of latitude (arc *A B*).

RULE VII.—The Earth is *depressed* at the poles, if the linear dimensions of the single degrees of any given meridian arc of 90° successively *de*crease (as do the linear dimensions of the arcs *A b″*, *b″ c″*, ... *f″ p′*) from 0° towards 90° of latitude, or from the equator towards the poles (by Rule II.).

RULE VIII.—The Earth is *elongated* at the poles, if the linear dimensions of any given meridian arc of 90°, extending from the equator=0° of latitude to the pole=90° of latitude, are *greater* (arc *A p*) than the linear dimensions of an arc of 90°, as computed with the linear value of a meridian arc of one degree in 0° of latitude (arc *A B*).

RULE IX.—The Earth is *elongated* at the poles, if the linear dimensions of the single degrees of any given meridian arc of 90°, successively *in*crease (as do the linear dimensions of the arcs *A b′*, *b′ c′*, ... *f′ p*) from 0° towards 90° of latitude, or from the equator towards the poles (by Rule II.).

RULE X.—Any two arcs of circles, *f. i.*, of one degree, being of identical linear extent, have of necessity the same radius (by Rule I.).

RULE XI.—The linear dimensions of any two arcs of circles, *f. i.*, of one degree (such as the theoretical meridian arc of one degree in 0° of latitude, and the actual equatorial arc of one degree in 0° of latitude) *having the same radius* (*A C*) *in common, are of necessity identical* (by Rule I.).

The last rule demands some words of explanation. It is plain that, as each single point in any given meridian arc belongs to a different circle of parallel, of which the corresponding point in the polar axis of the Earth may be considered the center; so does each point of a meridian arc belong to a different meridian circle,—the center of which is the Earth's center,—because every circle is proportional to its radius, and every point in a meridian arc, marking a corresponding point of latitude, is the extreme point of a different radius. Hence, when in astronomy a certain linear value is ascribed to a meridian degree in some given degree of latitude, which marks a mere point without linear extension in the meridian arc, that value is not a real, but a theoretical one; and the meaning of the astronomical expression is that, if a circle were described with the terrestrial radius, appertaining to the given degree of latitude, one degree of the linear circumference of such a theoretical circle would have the linear value in question.

It is essential to a correct understanding of our problem by the general reader, that he should fully comprehend this. Whether the equatorial diameter of the Earth be longer or shorter than its polar diameter, is here a matter of indifference, inasmuch as the principle applies equally to both cases. We will, therefore, in accordance with the present astronomical theory, assume the Earth to be a spheroid of revolution, depressed at its poles. Then, a glance at Fig. 7 and Fig. 8 will show, that the actual circle of the parallel in 45° of latitude,—represented in Fig. 8, as viewed rom some point in e prolonged polar xis, — is described about the polar axis by the terrestrial radius vector to 45° of itude, Cn, at an in- ation of 45° to the

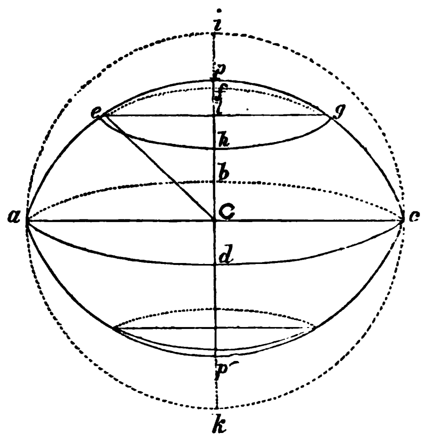


Fig. 7.

lane of the equator $a\,b\,c\,d$, and in a plane parallel with that plane. Hence, we may consider the same circle to be described with the right line $l\,e = C'\,e'$, Fig. 8, being a radius of that circle. And it is further seen, that the actual circle of the parallel in 0° of latitude, or of the equator $a\,b\,c\,d$, is described by the terrestrial radius vector to that latitude $C\,a = C'\,a'$, Fig. 8,—being the equatorial radius of the Earth—about the Earth's centre; and that the actual circle of the parallel in 90° of latitude or of the poles p and p', is described by the terrestrial radius vector to that latitude Cp or $Cp' = p'$, Fig. 8,—being the polar radius of the Earth—about itself. We may, therefore, look upon all circles of parallels,—a degree of each of which is one of 360 equal parts, into which those circles are divided,—as lying in the same plane of the equator, and having the same center, the center of the Earth,—being also the center of the normal circle, which serves to measure them—in common. To this common center, consequently, all radii, determining true degrees in either of the circles, must converge. For, from what has been previously explained, it is obvious that, if the center of the normal circle were to b applied to different points in any or all of those circle the result of such a measurement must necessarily be an erroneous one.

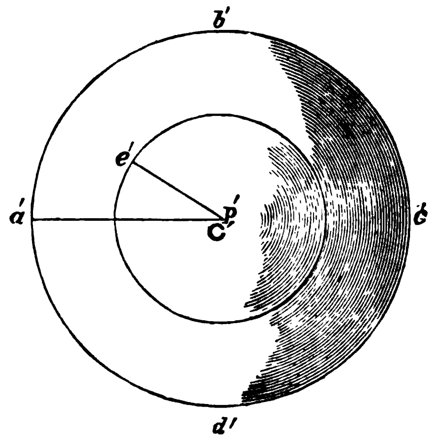


FIG. 8.

And this applies equally to meridian circles, though, as distinguished from circles of parallels, they are theoretical and not real circles. To render this plain, let us cast a glance at Fig. 9. It will be readily conceded that, supposing C a to be the Earth's equatorial radius, and the terrestrial globe to be a perfect sphere, determined by its equatorial extent, that the equatorial radius C *a* = C *a*, Fig. 7, = C′ *a′*, Fig. 8, describing the equatorial circle *a b c d* = *a i c k*, Fig. 7, = *a′ b′ c′ d′*, Fig. 8, will be the common measure of the entire terrestrial sphere. Or, supposing that sphere to be determined by the assumed polar radius C *p*, describing the circle *i p k p′*, it is plain that, in such a case, the latter radius is the common measure of the whole Earth. And again, if the terrestrial sphere were

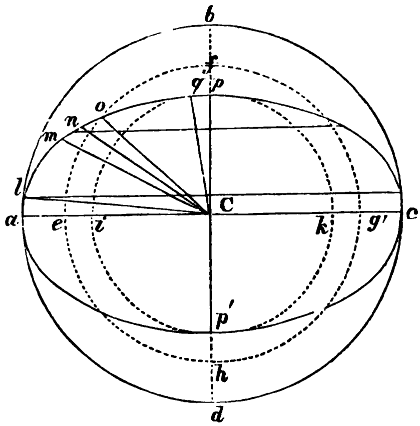


Fig. 9.

represented by the circle *e f g h*, its radius C *n*, being the radius vector to 45° of latitude, would be that common measure. Hence it is obvious, that the linear extent attributed to a meridian degree in 0°, 45°, 90° or any latitude whatever, is one of 360 equal parts of the linear circumference of circles, described with the terrestrial radius vector to the corresponding latitudes; and, since these circles do not coincide with the actual surface of the terrestrial spheroid in meridional directions, but deviate from it more or less, that they are theoretical and not real circles. Nor are, consequently, the linear dimensions of given meridian degrees, being portions of such circles, real dimensions, but theoretical ones. For instance, when a certain linear value is ascribed to an arc of one degree in 45° of latitude at *n*, which marks a mere point in the meridian, the meaning is not, that the given value represents the actual distance on the Earth's surface between 44° and 45° (*m* and *n*), or between 45° and 46° (*n* and *o*), or between $44\frac{1}{2}$° and $45\frac{1}{2}$° of latitude; but that it is the $\frac{1}{360}$th part of the linear extent of a theoretical circle *n f g h e*, supposed to be described with the terrestrial radius vector to 45° of latitude, C *n*. And the same, when a certain linear value is ascribed to an arc of one degree in 0° and in 90° of latitude, the meaning is, that the given linear values are the $\frac{1}{360}$th part of the linear extent in the former case of the theoretical circle *a b c d*, supposed to be described with the radius vector to 0° of latitude, being the Earth's equatorial radius; in the latter case, of the theoretical circle *i p k p′*, supposed to be described with the radius vector to 90° of latitude, being the polar radius of the Earth.

Each single point in the circumference of a meridian section of the Earth, therefore, belongs to a different circle, determined by the radius vector to that point; and the common center of all of which circles is the center of the Earth, being at the same time the center of the normal circle, by which degrees are measured on the Earth's surface.

Hence, too, it is manifest, as expressed by Rule XI., that, whatever be the true shape of the Earth, so long as it is regarded as a spheroid of revolution, the actual linear extent of its equatorial circumference $a\ i\ c\ k$, Fig. 7, $= a'\ b'\ c'\ d'$, Fig. 8, $= a\ c\ .\ \pi = a\ b\ c\ d$, Fig. 9, and the theoretical linear extent of its meridian circle $a\ b\ c\ d$, Fig. 7, $= a'\ b'\ c'\ d'$, Fig. 8, $= a\ b\ c\ d$, Fig. 9, *having the same radius* C a, Fig. 7, $=$ C′ a', Fig. 8, $=$ C a, Fig. 9, *in common*, are of necessity identical quantities. *And, consequently, the linear dimensions of a degree of the terrestrial equator and of a degree of any terrestrial meridian in* 0° *of latitude, being equal parts of equal circles,* ARE OF NECESSITY EQUAL QUANTITIES; *whether the Earth be flattened at its poles or elongated, or whether it be a perfect sphere.*

III.

The following table, taken from Dr. Maedler's popular work,* contains the principal elements of the Earth's figure, according to the results of actual geodetic measurements and the present astronomical theory:—

* "Popul. Astronomie," 5th ed. Berlin, 1861, p. 25.

I. Differences.	II. Geocentric Latitude.	III. Angle of the Vertical.	IV. Geograph. Latitude.	V. Length of a Meridian Degree.	VI. Length of a Degree of the Parallel.	VII. Radius Vector.
° ′ ″	° ′ ″	′ ″	°	Toises.	Toises.	
	0 0 0·0	0 0·0	0	56727·384	57108·520	1·000000
4 58 0·5	4 58 0·5	1 59·5	± 5	56731·698	56892·646	0·999975
4 58 4·0	9 56 4·5	3 55·5	10	56744·509	56246·572	0·999899
4 58 11·2	14 54 15·7	5 41·3	15	56765·440	55174·930	0·999778
4 58 21·6	19 52 37·3	7 22·7	20	56793·868	53685·416	0·999612
4 58 34·8	24 51 12·1	8 47·9	25	56828·948	51788·774	0·999407
4 58 50·8	29 50 2·9	9 57·1	30	56869·634	49498·744	0·999170
4 59 8·8	34 49 11·7	10 48·3	35	56914·708	46832·001	0·998907
4 59 28·5	39 48 40·2	11 19·8	40	56962·813	43808·110	0·998626
4 59 49·3	44 48 29·5	11 30·5	45	57012·498	40449·372	0·998336
5 0 10·0	49 48 39·5	11 20·5	50	57062·257	36780·748	0·998045
5 0 30·8	54 49 10·3	10 49·7	55	57110·576	32829·699	0·997763
5 0 50·6	59 50 0·9	9 59·1	60	57155·973	28625·998	0·997499
5 1 8·9	64 51 9·8	8 50·2	65	57197·058	24201·533	0·997259
5 1 25·1	69 52 34·9	7 25·1	70	57232·562	19590·076	0·997052
5 1 38·8	74 54 13·7	5 46·3	75	57261·389	14827·010	0·996884
5 1 49·3	79 56 3·0	3 57·0	80	57282·645	9949·043	0·996759
5 1 56·7	84 57 59·7	2 0·3	85	57295·668	4993·901	0·996683
5 2 0·3	90 0 0·0	0 0·0	90	57300·056	0·000	0·996657

It was Sir Isaac Newton's THEORY *of the origin of the Earth*, which first *decided* its shape to be that of a spheroid of revolution, depressed at the poles.* That theory rests fundamentally on two naked assumptions —namely, that the substance composing our globe was primitively in a fluid state; and that the naturally globular form of the Earth was, by its centrifugal force, flattened at the poles. The former assumption is contrary to all analogy, and, in the case of the Moon, found to be untenable by analysis itself.† The second assumption rests solely on an erroneous application

* At the time of the publication of the "Principia," opinions on the subject of the true figure of the Earth were still divided, and "it was Newton's *theory*," to quote Dr. Maedler, "which first *decided* that the Earth is flattened at the poles."—(Maedler, in the "Wochenschrift für Astronomie," Jahrgang 1859, p. 406.)

Your own words are still more to the purpose:—"Newton was the first person," you observe, "who made a calculation of the figure of the Earth *on the theory of gravitation.* He took the following supposition *as the only one to which his theory could be applied.* He assumed the Earth to be fluid. This fluid matter he assumed to be equally dense in every part. . . For trial of his theory he *supposed the fluid Earth to be a spheroid* [*flattened at the poles*]. . . . *In this manner* Sir Isaac Newton *inferred* that the form of the Earth would be a spheroid, in which the length of the shorter is to the length of the longer or equatorial diameter in the proportion of 229 to 230." (Airy, "Six Lectures on Astronomy," 4th ed., p. 194.)

† Laplace, "Exposition du Système du Monde" (1826), vol. ii. p. 250; Nicollet, "Mémoire sur la Libration de la Lune," Connaissance des Tems, 1822, p. 279; Poisson, "Connaissance des Tems," 1822, p. 283.

of terrestrial phenomena, depending on the repulsive force of our atmosphere and the attractive force of the Earth, to cosmical phenomena not subject to the action of any corresponding forces.* The analogy, therefore, fails. Sir Isaac Newton, who had a strong propensity to generalize, derived his speculative idea from the polar depression of Jupiter †—the analogy of which adduced by him is destroyed by the figures of the Moon and the Sun;‡—and then sought to establish a polar depression of the Earth also. But, independently of his erroneous assumptions to this effect, he rests his final result on the basis of two further erroneous elements and another arbitrary supposition. These elements are, that he takes the *mean* length of a meridian degree at 57061 toises, and the period of the Earth's rotation about itself at

* Moreover, you yourself say, with reference to Sir Isaac Newton's "centrifugal force:"—"*Centrifugal force is a fiction*; there is really no such thing as centrifugal *force*: the only idea which the term correctly can be intended to convey is, that in order to preserve a particle at one uniform distance from a center, when revolving round it, a certain *centripetal force* must be introduced." (Airy, "Mathem. Tracts," 4th ed., p. 140, note.) To say nothing of your definition itself, it is plain that, however you may define "a fiction," the definition must leave it A FICTION.

† "Principia," book iii. prop. xviii. and xix.

‡ Maedler, "Popul. Astronomie," p. 117. The fact of the Sun's polar elongation has been questioned and attributed to "a deception of some kind;" but solely because "it is difficult to reconcile the fact with the laws of gravitation."

$23^{h.}\ 56^{m.}\ 4^{s.}$ * The supposition is *a fundamental proportion of the Earth's polar axis to its equatorial*

* It is easy to show that the time which elapses between two successive culminations of the same *star* for a given meridian of the Earth is *not* the true period of the Earth's daily motion about itself.

In order that the general reader may be able to judge of the whole argument of Sir Isaac Newton, I will quote of the prop. xix. of book iii. of the "Principia," what relates to the figure of the Earth :—

"*To find the proportion of the axis of a planet to the diameters perpendicular thereto* :—Our countryman Mr. Norwood, measuring a distance of 905,751 feet of London measure between London and York, in 1635, and observing the difference of latitudes to be 2° 28′, determined the measure of one degree to be 367,196 feet of London measure, that is 57,300 Paris toises. M. Picart, measuring an arc of one degree and 22′ 55″ of the meridian between Amiens and Malvoisine, found an arc of one degree to be 57,060 Paris toises. M. Cassini, the father, measured the distance upon the meridian from the town of Collioure, in Roussillon, to the Observatory of Paris; and his son added the distance from the Observatory to the citadel Dunkirk. The whole distance was $486,156\frac{1}{2}$ toises; and the difference of the latitudes of Collioure and Dunkirk was $8^\circ\ 31'\ 11\frac{5}{6}''$. Hence, an arc of one degree appears to be 57,061 Paris toises. And from these measures we conclude that the circumference of the Earth is 123,249,600, and its semi-diameter 19,615,800 Paris feet, upon the supposition that the Earth is of a spherical figure.

"In the latitude of Paris a heavy body falling in a second of time describes 15 Paris feet, 1 inch, $1\frac{7}{9}$ line, as above, that is, $2,173\frac{7}{9}$ lines. The weight of the body is diminished by the weight of the ambient air. Let us suppose the weight lost thereby to be $\frac{1}{11000}$ part of the whole weight; then that heavy body falling *in vacuo* will describe a height of 2,174 lines in one second of time. A body in every sideral day of 23h. 56m. 4s. uniformly revolving in a circle at the distance of 19,615,800 feet from the center, in one second of time describes an arc of 1433·46 feet, the versed sine of *diameter of* 100 : 101. On the ground of all these purely imaginary or positively erroneous elements,

which is 0·05236561 feet, or 7·54064 lines. And therefore the force with which bodies descend in the latitude of Paris is to the centrifugal force of bodies in the equator arising from the diurnal motion of the Earth as 2174 to 7·54064.

"The centrifugal force of bodies in the equator is to the centrifugal force with which bodies recede directly from the Earth in the latitude of Paris 48° 5′ 10″ in the duplicate proportion of the radius to the cosine of the latitude—that is, as 7·54064 to 3·267. Add this force to the force with which bodies descend by their weight in the latitude of Paris, and a body in the latitude of Paris falling by its whole undiminished force of gravity, in the time of one second, will describe 2,177·267 lines, or 15 Paris feet, 1 inch, and 5·267 lines. And the total force of gravity in that latitude will be to the centrifugal force of bodies in the equator of the Earth as 2177·267 to 7·54064, or as 289 to 1.

"Wherefore, if *A P B Q* Fig. 10, represent the figure of the Earth, now no longer spherical, but generated by the rotation of an ellipsis about its lesser axis *P Q;* and *A C Q q c a*, a canal full of water, reaching from the pole *Q q*, to the center *C c*, and thence rising to the equator *A a;* the weight of the water in the leg of the canal *A C c a*, will be to the weight of water in the other leg *Q C c q*, as 289 to 288, because centrifugal force arising from the circular motion sustains and takes off one of the 289 parts of the weight in the one leg), and the weight of 288 in the other sustains the rest. But by computation (from Cor. 2, prop. xci., book i.) I find, that, if the matter of the Earth were all uniform, and without any motion, *and its axis P Q were to the diameter A B as* 100 *to* 101, the force of gravity in the place *Q* towards the Earth would be to the force of gravity in the same place *Q* towards a sphere described about the center *C* with the radius *P C* or *Q C*, as 126 to 125. And, by the

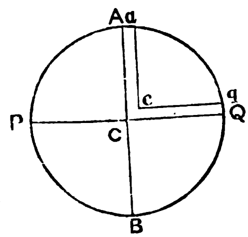


FIG. 10.

Sir Isaac Newton, by a strange compound of proportions, arrives at the ultimate conclusion

same argument, the force of gravity in the place *A* towards the spheroid generated by the rotation of the ellipsis *A P B Q* about the axis *A B* is to the force of gravity in the same place *A*, towards the sphere described about the center *C*, with the radius *A C*, as 125 to 126. But the force of gravity in the place *A* towards the Earth is a mean proportional betwixt the forces of gravity towards the spheroid and this sphere; because the sphere, *by having its diameter* P Q *diminished in the proportion of* 101 *to* 100, *is transformed into the figure of the Earth;* and this figure, by having a third diameter perpendicular to the two diameters *A B* and *P Q* diminished in the same proportion, is converted into the said spheroid; and the force of gravity in *A*, in either case, is diminished nearly in the same proportion. Therefore, the force of gravity in *A* towards the sphere described about the center *C*, with the radius *A C*, is to the force of gravity in *A* towards the Earth as 126 to 125½. And the force of gravity in the place *Q* towards the sphere described about the center *C* with the radius *Q C*, is to the force of gravity in the place *A* towards the sphere described about the center *C*, with the radius *A C*, in the proportion of the diameters (by Prop. lxxii. book 1), *that is, as* 100 *to* 101. If, therefore, we compound those three proportions, 126 to 125, 126 to 125½, and 100 to 101, into one, the force of gravity in the place *Q* towards the Earth will be to the force of gravity in the place *A* towards the Earth, as $126 \times 126 \times 100$ to $125 \times 125\frac{1}{2} \times 101$, or as 501 to 500.

"Now, since (by Cor. 3, prop. xci., book i.) the force of gravity in either leg of the canal *A C c a*, or *Q C c q*, is as the distance of the places from the center of the earth, if those legs are conceived to be divided by transverse, parallel, and equidistant surfaces into parts proportional to the wholes, the weights of any number of parts in the one leg *A C c a* will be to the weights of the same number of parts in the other leg as their magnitudes and the accelerative forces that the polar depression of the Earth amounts to $\frac{1}{230}$.*

of their gravity conjunctly; that is, as 101 to 100, and 500 to 501, or as 505 to 501. And, therefore, if the centrifugal force of every part in the leg *A Cc a*, arising from the diurnal motion, were to the weight of the same part as 4 to 505, so that from the weight of every part, conceived to be divided into 505 parts, the centrifugal force might take off four of those parts, the weights would remain equal in each leg, and therefore the fluid would rest in an equilibrium. But the centrifugal force of every part is to the weight of the same part as 1 to 289; that is, the centrifugal force, which should be $\frac{4}{505}$ parts of the weight, is only $\frac{1}{289}$ part thereof. And therefore I say, by the rule of proportion, that if the centrifugal force $\frac{4}{505}$ make the height of the water in the leg *A Cca* to exceed the height of the water in the leg *Q Ccq* by one $\frac{1}{100}$th part of its whole weight, the centrifugal force $\frac{1}{289}$ will make the excess of the height in the leg *A Cca* only $\frac{1}{289}$ part of the height of the water in the other leg *Q Ccq;* and *therefore* the diameter of the Earth at the equator is to its diameter from pole to pole as 230 to 229."

Such is the dropsical, rather than the "mathematical" *foundation* of the theory of modern astronomy concerning the true figure of the Earth. One of its principal elements is *the naked pre-assumption of a polar depression of* $\frac{1}{100}$. If the reader will bear this in mind, and whenever the expression "centrifugal force" occurs in Sir Isaac Newton's argument, substitute for it *your own definition*—"A FICTION," he will, without reference to minor things, arrive at a pretty correct conclusion as to the "mathematical" value of that theory.

* Huyghens, who, consistently at least, and without *pre-supposing* a polar depression of the Earth, found this depression to be only $\frac{1}{578}$, remarks upon the result arrived at by Sir Isaac Newton:—"Newtonus excessum hunc $\frac{1}{578}$ invenit $\frac{1}{231}$ ac proinde figuram terræ multo magis distare a sphærica. Quo in calculo *utitur quâdam supputandi ratione,* quam hic non examinabo, *siquidem haud assen-*

Subsequent pendulum experiments and geodetic measurements chiefly of meridian arcs, though calculated upon the Newtonian theory itself and by the aid of purely empirical and tentative formulas, have shown that conclusion to require an essential modification; the former indicating an ellipticity of $\frac{1}{288}$ nearly,* the latter an ellipticity of about $\frac{1}{299}$. For the purpose of adapting the Newtonian result to the ellipticity derived from geodetic measurements, recourse is had to the *interior* condition of the Earth; in other words, the *heterogeneous* element of the Earth's interior is *imagined* and introduced into the higher calculus, as a mere mathematical variable quantity, for the reduction of Sir Isaac Newton's *theoretical* ellipticity of $\frac{1}{230}$ to what is at various times, or by various processes found and believed to be the *empirical* ellipticity of our globe.† On the

tior illi axiomati quod ibi aliasque sumit."—Huyghens, "De Causâ Gravitatis," Opera reliqua, Amstel. 1728, 4to. vol. i., p. 121.

* Von Humboldt's "Cosmos," vol. iv. General Sabine's "Note on the Ellipticity of the Earth," p. 458.

† With reference to the ellipticity of the Earth of $\frac{1}{230}$, found by the author of the "Principia," you yourself remark, that in this "Newton was wrong;" but, you add, "When we consider the matter, it is very unlikely that, *if* the interior of the Earth is fluid, its density is equal in every part," and "except you know what is the structure of the interior, you cannot say what the ellipticity of the Earth will be." Hence, the disciples of Sir Isaac Newton supply the "knowledge" of which their master was deficient, and bring to

other hand, the discrepancy between both and the polar depression, resulting from pendulum expe-

the aid of his theory the further *supposition* (and which is really the only kind of supposition to be made at all in this investigation) that the [fluid] Earth consists of strata of different densities, but that each stratum is in some degree elliptical; the ellipticity of one stratum being different from that of another." (Airy, "Six Lectures on Astronomy," 4th ed., p. 202-3.)

As to the "heterogeneous" element of the Earth, the theorem of Clairaut may serve us to fix its analytical meaning. You say ("Figure of the Earth," p. 188):—"The co-efficient of the square of the sine of latitude is $2e - \frac{9}{5} \cdot \frac{\psi(b)}{b^2\phi(b)} + m$. Now (from 63) we get $\frac{9}{5} \cdot \frac{\psi(b)}{b^2\phi(b)} = 3e - \frac{3m}{2}$. Substituting, the co-efficient of $\sin^2\lambda$ is $\frac{5m}{2} - e$; and gravity is now expressed by $E\left(1 + \frac{5m}{2} - e \cdot \sin^2\lambda\right)$, a remarkably simple expression, all functions, and, in fact, everything depending on the internal constitution of the spheroid having disappeared. It appears from this, that if by observations of the force of gravity at different latitudes we can express it by a formula of this kind $E(1 + F\sin^2\lambda)$, then e, the ellipticity of the Earth's surface, will be $= \frac{5m}{2} - F$. This is the celebrated theorem known by the name of "Clairaut's Theorem."

It is generally admitted to be a "remarkable" theorem, *i. e.*, a theorem, the *meaning* of which no astronomer understands. It need not be observed, that it applies to a homogeneous Earth as well. You expressly say, moreover, "Let this $\left(\frac{5m}{2} - e\right) = n$: then $n + e = \frac{5m}{2}$: a very remarkable proposition, which may be thus stated: *Whatever be the law of the Earth's density*, if the ellipticity of the surface be added to the ratio which the excess of the polar above the equatorial gravity bears to the equatorial gravity, their sum will be $\frac{5m}{2}$, m being the ratio of the centrifugal force" [according to your definition "a

D

riments, is left unexplained, for the simple reason that astronomy has not even a conjecture to offer as to its cause,* though a mere error of calculation. Another support, however, in favour of the approximate ellipticity of the Earth of $\frac{1}{300}$ is said to be derived from certain inequalities in the Moon's motion, supposed to depend on the Earth's polar depression; and as that inequality, you remark,† "depends on $e - \frac{m}{2}$, it gives us the means of ascer-

fiction"] "at the equator to the equatorial gravity."—*Mathem. Tracts*, *4th ed. p.* 172.

Now, if we write with you, $\frac{5m}{2} - e = F$ then $F + e = \frac{5m}{2}$; but e, the homogeneity of the Earth being assumed, $= \frac{5m}{4}$, and, therefore, $F = \frac{5m}{4}$. As m, however, has the *constant* value of $\frac{1}{289}$, for the homogeneous Earth $e + F$ is $= \frac{5m}{2} = \frac{1}{115}$, and gravity $= E\left(1 + \frac{e+F}{2} \sin 2\lambda\right)$. But since the general expression for gravity $= E\left(1 + \frac{5m}{2} - e \cdot \sin {}^2\lambda\right)$, in which $\frac{5m}{2} - e$ has a constant value, likewise applies to a homogeneous Earth, $\frac{e+F}{2}$ is $= \frac{5m}{2} - e$, and, consequently, has the same constant value of $\frac{1}{230}$, dependent on $m = \frac{1}{289}$. Hence, the theorem of Clairaut, translated into vulgar language, means simply this, that modern astronomy and analysis, with Sir Isaac Newton, assume the Earth to be homogeneous, and its ellipticity to be $\frac{1}{230}$; but, as from geodetic measurements they deduce the ellipticity to be only about $\frac{1}{300}$, that they *imagine* the heterogeneity of the Earth, to serve as a purely theoretical and variable element of calculation for the purpose of reducing the former value to the latter, whatever

... a.

...ry, "Figure of the Earth," p. 231.

...igure of the Earth," p. 189.

taining the value of e (the ellipticity) *without* any knowledge of the internal constitution of the Earth." But this rests on an evident misapprehension on your part; for, independently of the purely imaginary character of the expression $e - \frac{m}{2}$ itself, it is plain, because the quantity of the ellipticity is taken to depend on the Earth's interior condition, that the quantity of its action upon the Moon must depend on its interior condition to a corresponding extent; and, consequently, if the astronomer is able to read in that action with perfect accuracy the Earth's true shape,* that he must be able to read, and unknowingly have read in it, as well, the interior condition of the Earth, on which its shape is said to depend. Indeed, you yourself state, that "*unless* you know what is the structure of the interior, you *cannot* say what the ellipticity of the Earth will be." †

My sole object, in submitting to you these remarks, has been to show that THE EARTH OF THE NEWTONIAN THEORY IS THE MERE CREATION OF

* Laplace, "Exposition du Système du Monde," p. 230; comp. von Humboldt's "Cosmos," vol. iv., p. 24, who remarks, "I recall with pleasure the happy expressions, by which the method was characterized by its inventor, 'that an astronomer, without quittin his observatory, is able to read in the motions of a single heave body the precise form of the Earth which he inhabits.'"

† Airy, "Figure of the Earth," p. 203; see the note to page 32

THE FANCY; *that its shape has been determined on the ground, partly of simply imaginary, partly of positively erroneous elements; and that the results of subsequent experiments and measurements have, by means of purely mathematical factors and tentative formulas, been adapted to its* PRESUPPOSED *figure.* The only difficulty, indeed, I shall have to overcome in establishing the Earth's *true* shape, will consist in making the astronomer and the analyst perceive that their *imagined* Earth is not the *real* Earth; and that, in order to behold its actual form, it is indispensable to *set astronomical theories and preconceived notions aside*, and to fix the eye exclusively on *empirical facts and geometrical principles.*

In the mean time, modern astronomy explains the empirical elements of Dr. Maedler's table on the ground of that purely imaginary shape, lent to the Earth by Sir Isaac Newton's theory of its origin. These elements are, or may be considered to be, those of the columns IV. and V. The elements of all the remaining columns are *computed.* Thus, the "length of a degree of the parallel," column VI., is not a measured, but a calculated length; and the few actual measurements of degrees of parallels, thus far carried out, exhibit, as compared with those calculated values, and already in middle latitudes, discrepancies so great, that astronomers have been led to look upon such measurements—though most unreasonably so—as of a less reliable nature, and therefore to be discountenanced. The "angle of the vertical," column III., also, and the "geocentric latitude," column II., as distinguished from the geographical latitude, together with their "differences," column I., are similar imaginary elements, computed on the ground of the Earth's *presupposed* polar depression. It is by means of the *angle of the vertical*, that astronomy converts the empirical length of an equatorial meridian degree of 56727·384 toises, according to the approximate proportion of 0° 59′ 36″: 1° 0′ 0″, into the theoretical length of 57108·520 toises; and what may be considered the empirical length of a polar meridian degree of 57300·056 toises, according to the inverse proportion of 1° 0′ 0″ : 0° 59′ 36″, into the theoretical length of 56918·05 toises, adapting the "radius vector" of the Earth, column VII., to those purely theoretical values, instead of deducing it from the actual linear extent of the terrestrial surface. Thus, *the only* EMPIRICAL *elements of our Table are seen to be the geographical latitudes*, column IV., *and the lengths of meridian degrees*, column V.,—a circumstance which should be borne in mind.

IV.

If we conceive a great circle in the heavens, the 360 radii of which converge towards, and meet in, the center of the Earth, this will be the normal circle, by which true degrees are, and alone can be, determined on the terrestrial surface, intersected by those radii. Practically, the points of intersection are determined by the plumb-line. Supposing now the Earth to be a perfect sphere; it is universally admitted that, in such a case, all plumb-lines or *normals*, prolonged, would meet in the Earth's center, and, consequently, coincide with the radii of the normal circle, determining in a direct manner true degrees on the terrestrial surface. And, therefore, assuming the figure of the Earth to slightly deviate from that of a perfect sphere, it is natural to conclude, *without a positive proof or reason to the contrary*, that the plumb-lines would continue to be directed to the Earth's center, all the same. Astronomy, however, not only without any proof or reason whatsoever, *assumes* that they do not; but, moreover, *starting* on the assumption that the *imaginary* shape, lent to the Earth by Sir Isaac Newton's theory, is its *real* shape, gives to the plumb-lines *such imaginary directions, as are needed in order to adapt the empirical results of geodetic measurements to the Earth's imagined form.*

To show this, I will quote your own words:—"And no two expeditions"—speaking of the French scientific expeditions to Lapland and to Peru, you say *—"ever rendered themselves more justly celebrated than these. Now observe the results. In Fig. 11, *A B* represents the Lapland measure, *a b* the Peruvian measure. It was found, that in Lapland they had to go 69¾ miles, or something like that, in order

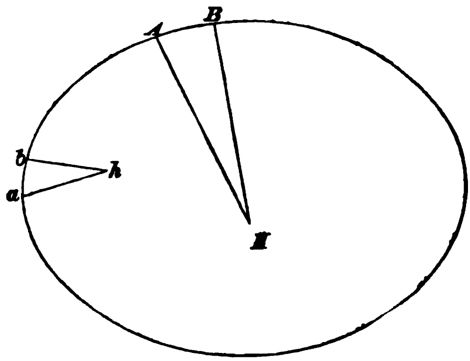


FIG. 11.

that the directions of the verticals should change one degree. It was found in Peru that they had to go only 69 miles, in order that the direction should change one degree. *From this it follows* that the verticals *A H* and *B H* in Lapland meet at a point *H*, whose distance from *A* or *B* is about 4,000 miles; and that the verticals *a h* and *b h* in Peru meet at a point *h*, whose distance from *a* or *b* is about 3,950 miles."

"What," you continue, "is to be inferred from this? We have said that the estimation of the semi-diameter of the Earth, supposing it to be a sphere, would depend upon the distance you have to go, in order that the direction of the vertical might be altered by one degree. We have to go further in the northern measure than in the equa-

* Airy, "Six Lectures on Astronomy," 4th ed. p. 46, 47.

torial measure. *It would seem at first sight, as a consequence, that the Earth was egg-shaped* [elongated at the poles]; and this was maintained by many respectable people at the time. *On consideration,* it appeared that this was not a correct inference. And the *reasons* were these: *when we assume* that the Earth is an ellipse, not a sphere, then, *inasmuch as we mean by the direction of gravity the 'direction of a line perpendicular to the surface of the water,'* the direction of gravity will not go to the Earth's center at all. It is *necessary to consider something different;* and that is, that the measures which we have obtained give us information of the *curvature* of different points of the Earth. They tell us, that at *A B* the curvature is little, but that at *a b* the curvature is very sharp. *Altogether, when properly considered,* they lead us to the *inference* that the form of the Earth is *something like the oval in Fig.* 11; that it is *flatter at the poles, and sharper in its curvature at the equator.* The rule which *theory* gave was, that the Earth would be spheroidal; *that is,* that its form would be that which is produced by an ellipse revolving round its *shorter* axis. *Adopting, then, the supposition that the Earth is spheroidal*—[*'that is'* that its axis of rotation, i.e., *that its polar axis is shorter*]—it was a matter of calculation to determine from the geometrical properties of the ellipse, *what would be the proportion of the two axes of the Earth, which could make the proportions of the* CURVATURES *at A B and a b similar to those determined from the observations.* It was inferred that they were in a proportion something like 299 to 300."

From this statement, the logic of which I will leave to speak for itself, two things appear perfectly clear. The first is that, as I have said, the deviation of the plumb-line from one common direction to the center of the Earth, is a *mere* assumption; and that this assumption is applied to the purely theoretical and *presupposed* form of the Earth, as that of a spheroid of revolution depressed at the poles to the extent of $\frac{1}{300}$. But with precisely the same right and reason we might assume the Earth's shape to be that of *any spheroid of revolution whatsoever,* and then adapt to the *chosen* shape the results of geodetic measurements, by means of imaginary angles of the vertical and arbitrary analytical formulas.

The second point you state, deserving of special notice, is, that a first and *still unbiassed* judgment would, from the results of geodetic measurements, naturally conclude the Earth to be *elongated* at the poles; but *when the poles of the Earth are assumed to be depressed,* that *then* also it becomes necessary to *assume* the directions of the plumb-line to deviate from one common direction to the Earth's center, and to be *what that imagined shape implies them to be;* in short, that we have to deduce the *true* figure of the Earth *from the curvature of its* PRESUPPOSED *curvature.*

And such, indeed, is not only the vicious reasoning, but also the vicious mathematical process, by which modern astronomy and analysis have endeavoured to prove the Earth's polar depression, the mère creation of Sir Isaac Newton's speculative theory of its origin, to be an empirical fact.

V.

Let us first cast a glance at the *general* principles, which are adduced in support of the pretended fact, to which I have just alluded. Sir John Herschel explains and argues thus:*—" It is evident from a mere inspection of this table "—referring to a table similar to Professor Maedler's—" that *the measured length of a degree increases with the latitude,* being greatest near the poles and least near the equator. Let us now consider what interpretation is to be put upon this conclusion as regards the form of the Earth.

" Suppose we held in our hands a model of the Earth smoothly turned in wood: it would be so nearly

* " Outlines," p. 135.

spherical that neither by the eye nor the touch, unassisted by instruments, could we detect any deviation from that form. Suppose, too, we were debarred from measuring directly across from surface to surface in different directions with any instrument, by which we might at once ascertain whether one diameter were longer than another: how, then, we may ask, are we to ascertain whether it is a true sphere or not? It is clear that we have no resource but to endeavour to discover, by some nicer means than simple inspection or feeling, whether the convexity of its surface is the same in every part; and if not, where it is greatest and where least. Suppose, then, a thin plate of metal to be cut into a concavity at its edge, so as exactly to fit the surface at *A*: let this now be removed from *A*, and applied successively to several other parts of the surface, taking care to keep its plane always on a great circle of the globe, as represented in Fig. 12. If, then, we find any position, *B*, in which the light can enter in the middle between the globe and the plate, or any

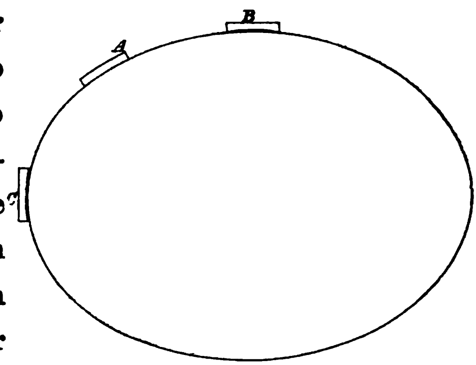


Fig. 12.

other, *C*, where the latter tilts by pressure, or admits the light under its edges, we are sure that the *curvature* of the surface at B is less, and at C greater, than at *A*."

"What we here do by the application of a metal plate of determinate length and curvature, we do on the Earth by the measurement of a degree of variation in the altitude of the pole. Curvature of a surface is nothing but the continual deflection of its tangent from one fixed direction, as we advance along it. When, in *the same measured distance of advance* we find the tangent (which answers to our horizon) to have shifted its position with respect to a fixed direction in space (such as the axis of the heavens, or the line joining the Earth's centre and some given star) *more* in one part of the Earth's meridian than in another, we conclude, of necessity, that the curvature of the surface at the former spot is greater than at the latter; and *vice versâ*, when, in order to produce the same change of horizon with respect to the pole (suppose 1°) we require to travel over a *longer* measured space at one point than at another, we assign to that point a less curvature. Hence we conclude that *the curvature of a meridional section of the Earth is sensibly greater at the equator than towards the poles;* or, in other words, that the Earth is not spherical, but *flattened* at the poles, or, which comes to the same, protuberant at the equator."

This argument contains two or three essential errors, intimately connected with each other. The first consists in Sir John Herschel's definition of "curvature of a surface," which makes it to depend on some arbitrarily chosen fixed direction *without*, instead of a given fixed point, the center, *within* the circumference. The second error is embodied in the corresponding assertion, by implication, that (true) degrees (of a circle) are directly determined on elliptical surfaces by the corresponding variation in the latitudinal direction of a metal plate, meridionally applied to such surfaces. Hence, the logic of the argument is inverted, leading to the final error (as proved by Rules iii. and iv.), of a conclusion, diametrically opposed to the truth, being drawn from correct premises.

The reason of Sir John Herschel's first error is, that astronomy neglects to distinguish between the circular curvature in an ellipse, and the elliptical curvature of the ellipse. Where the former is greatest, the latter is least; and *vice versâ*. This will appear evident on our casting a glance at the diagram, Fig. 13. At the extreme point A of the minor axis $A\ E$ of the ellipse $A\ B\ E\ D$ the circular curvature $c\ c'$ in the ellipse,—proportional to the radius $A\ C$

and the circle *A F E G*, described with it,—as measured by the right line *a a′*, is greatest; while

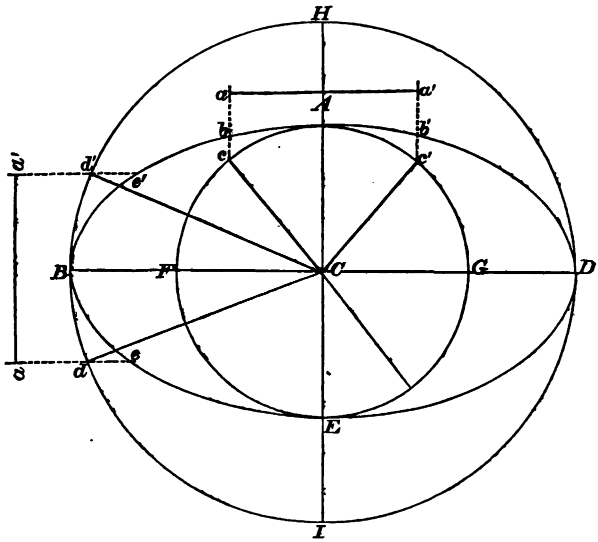


FIG. 13.

the corresponding elliptical curvature *b b′* is least. On the contrary, at the extreme point *B* of the major axis *B D* of the ellipse, the circular curvature *d d′* —proportional to the radius *B C* and the circle *B H D I*—as measured by the same right line *a a′*, is least; while the corresponding elliptical curvature *e e′* is greatest. Hence, the circular curvature of the Earth's surface, being found less, and its elliptical curvature greater at the poles than at the equator, the poles are elongated.

Under the common misapprehension, Arago, in his

"Astronomie Populaire,"* which has been translated into our own language by Admiral Smyth and Professor Grant, for the purpose of explaining the polar depression of the Earth, states:—"If we wish to determine the form of a line or any curved surface, the most direct method consists in letting perpendiculars, by geometers termed *normals*, fall upon it. Where the curvature is greatest, we have to move only a comparatively short distance along the circumference of the curved line or the curved surface, to cause the normals to our point of departure and to the station we have reached, to form an angle, say of one degree. Where the curvature is least, the distance we have to go in order to make the normals form an angle of 1° is greater than in the previous case. The reader will readily apprehend this, on casting a glance at Fig. 14, which shows that, for the same angle O, formed by the normals *N* and *N′*, *M* and *M′*, the distance *A B* is much greater than the distance *C D*, because the curvature at *A B* is much less than it is at *C D*."

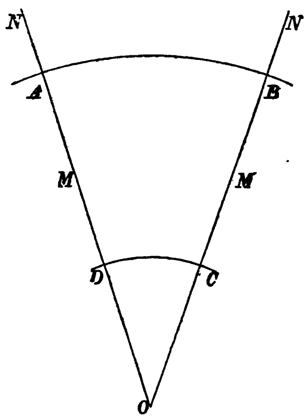


FIG. 14.

"The distance on a body's surface, comprised between two normals, forming a certain angle, is the greater the smaller the curvature of the surface is."

* Vol. iii. (Paris 1856), p. 7.

All this is as obviously true as the inference drawn from it by Arago, namely a polar depression of the Earth, is erroneous; for, the empirical fact being the gradual decrease of the circular curvature of the terrestrial surface from the equator towards the poles, it follows (by Rule iv.), that the surface of the Earth, having less circular curvature at the poles than at the equator, is at the equator less distant from the Earth's center than at the poles, *i.e.*, that the Earth is elongated at the poles. Indeed, a mere inspection of Arago's illustrative diagram renders it evident, that (in conformity with Rules ii. and iii.) the greater linear dimensions, *A B*, of a meridian degree and the smaller curvature of the Earth's surface at the poles, as compared with the corresponding elements *D C* at the equator, show the polar radius $B\,O = A\,O$ to be greater than the equatorial radius $D\,O = C\,O$; and, consequently, prove the polar elongation of the Earth.

The second error of principle, implied by Sir John Herschel, I may be permitted to illustrate by a practical example, which at the same time will serve to make us clearly see, by which process it is that modern astronomy converts the polar elongation of the Earth into a polar depression. Let us suppose Fig. 15 to represent the actual shape of an oval lamp-screen or any similar object, bodily placed before us, so that we are certain of the *fact* of its major axis being perpendicular to its stand and to the plane of

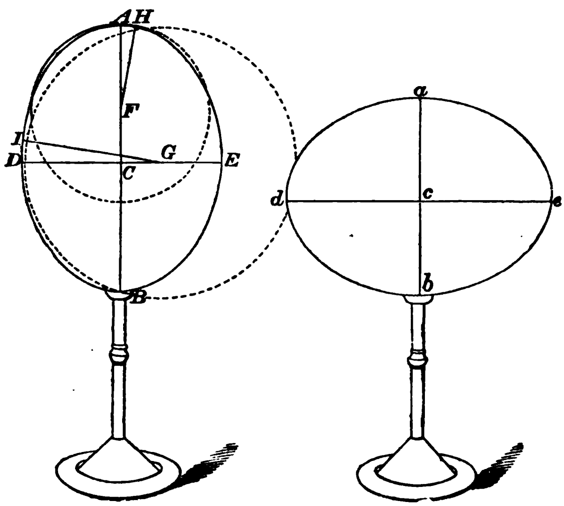


FIG. 15.

A B } the major axis } of the
d e }
D E } the minor axis } screen.
a b }

FIG. 16.

A H } arcs of one degree each.
I D }
F, G—centres of curvature.
A F, D G—radii of curvature.

the table. Now, astronomy will furnish the reasoning, and analysis will supply the "mathematical proof" to show, that the position of our screen is *not* what it actually *is*, but what it is represented to be in Fig. 16. The process is this. The greater curvature at *A* (Fig. 15), it is argued, belongs to the smaller arc of 1°, *A H*, of a small circle, of which *F* is the center and *A F* the radius; hence, the distance of *A* from the center *C* is = *A F;* but *A F* (Fig. 15) = *a c* (Fig. 16), which, consequently, is the distance of $A = a$ from the center $C = c$, or the radius vector to the point $A = a$. Again, the smaller curvature at D (Fig. 15) belongs to the greater arc of 1°, $D\,I$, of a greater circle, of which G is the center and $D\,G$ the radius: hence, the distance of D from the center C is $= D\,G$; but $D\,G$ (Fig. 15) $= dc$ (Fig. 16), which, consequently, is the distance of $D = d$ from the center $C = c$, or the radius vector to the point $D = d$. Therefore, our elliptical or oval screen is not placed on its stand with its major axis perpendicular to the table (as it actually is); but with its minor axis perpendicular to the table (as represented in Fig. 16, and as it is not).

The astronomical error lies in the assumption, that the elliptic arcs $A\,H$ and $D\,I$ are arcs of two circles, the centers of which are F and G, distant from each other and from the real center C; whereas they are not—every point in either arc belonging, as previously explained, to a different circle, the center of which, common to the whole number of these circles, is C.

VI.

But, however simple and striking in principle, the astronomical error, as applied to the actual problem of the Earth's true figure, is of a more complicated character. In order to render it generally intelligible, I will first quote the words, in which Sir John Herschel proceeds to more fully demonstrate the polar depression of the Earth. "Let *N A B D E F* (Fig. 17) represent," he writes,* "a meridional section of the Earth, *C*

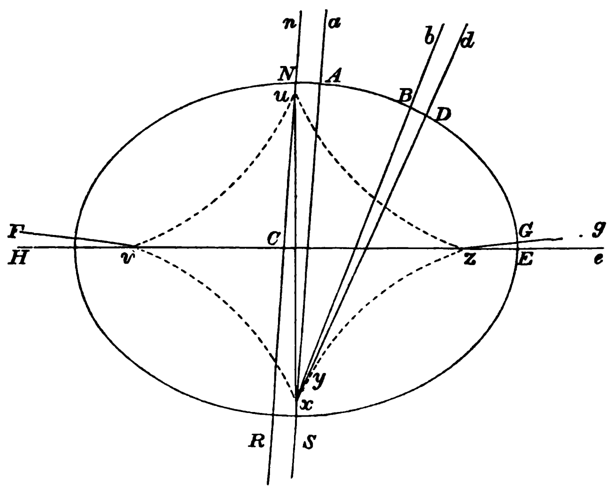


FIG. 17.

its center, and *N A*, *B D*, *G E* arcs of a meridian, each corresponding to one degree of difference of latitude, or to one degree of variation in the meridian altitude of a star, as referred to the horizon of a spectator travelling along the meridian. Let *n N*, *a A*, *b B*, *d D*, *g G*, *e E*, be the respective directions of the *plumb-line* at the stations *N*, *A*, *B*, *D*, *G*, *E*, of which we will suppose *N* to be at the pole and *E* at the equator;

* "Outlines," p. 137. The illustrative diagram (Fig. 17) given by Sir John Herschel has not the lines *u R* and *v H*, which have been added.

then will the tangents to the surface at these points respectively be perpendicular to these directions; and, consequently, if each pair, viz., *n N* and *a A*, *b B* and *d D*, *g G* and *e E*, be prolonged till they intersect each other (at the points *x*, *y*, *z*), the angles *N x A*, *B y D*, *G z E*, will each be one degree, and, therefore, all equal; so that the small curvilinear arcs *N A*, *B D*, *G E*, may be regarded as arcs of circles of one degree each, described about *x*, *y*, *z* as centers. These are what in geometry are called *centers of curvature*, and the radii *x N* or *x A*, *y B* or *y D*, *z G* or *z E*, represent *radii of curvature*, by which the curvature at those points is determined and measured. Now, as the arcs of different circles, which subtend equal angles at their respective centers, are in the direct proportion of their radii, and as the arc *N A* is greater than *B D*, and that again than *G E*, it follows that the radius *N x* must be greater than *B y*, and *B y* than *E z*. *Thus it appears* that the mutual intersections of the plumb-lines will not, as in the sphere, all coincide in one point *C*, the center, but will be arranged along a certain curve, *x y z* (which will be rendered more evident by considering a number of intermediate stations). To this curve geometers have given the name of the *evolute* of the curve *N A B D G E*, from whose centers of curvature it is constructed."

That the direction of the plumb-lines or normals to any given point on the Earth's surface is perpendicular to a tangent to that point or to the plane of its horizon, is, as I have already shown, and as appears also distinctly from Sir John Herschel's own words, a mere assumption, unsupported by even the shadow of a reason; for what possible connection can there be between the positive force, or a law of nature, which determines the directions of the plumb-line, and the *imaginary* line and plane, which astronomers term "a tangent" and "the horizon?"

For arguments' sake, however, let us for one moment admit the principle. In that case it is evident, with reference to any given spheroid of revolution, whether it revolve about its major or its minor axis, firstly, that normals to any point of its equator and to its poles will coincide with the corresponding radii of a normal circle, the center of which is the given body's center, or with the corresponding normals to the surface of a perfect sphere; and secondly, that normals to any other point on the spheroid's surface will deviate from the corresponding normals to a perfect sphere, *in the same proportion as the given spheroid itself deviates from such a sphere.* We cannot possibly, therefore, calculate these deviations or "angles of the vertical," unless the spheroid, to which they are to apply, is *given;* and hence it is obvious that these angles, or the *imaginary* differences between the real geographical latitudes and the theoretical geocentric latitudes of Dr. Maedler's table, apply only to the *imaginary* shape of the Earth of Sir Isaac Newton's theory; and that the only use made of them in astronomy, is to adapt the Earth's actual proportions, or what is known of them, to that imagined shape.

To test, however, the truth of the process and the theory, by reversing the assumption, as the practical astronomer reverses his instrument, has not been thought of. Yet, it is manifest that, if the process be correct, the same elements, which show a spheroid to deviate from a sphere to the extent of $\frac{1}{300}$ in the direction of the equator, should, when applied inversely, show a deviation to the *same* extent in the direction of the poles. But we find :—

	Empirical length.	Proportions of Reduction.*	Theoretical length. Assuming polar depression.	Theoretical length. Assuming polar elongation.
	Toises.	° ′ ″ ° ′ ″	Toises.	Toises.
Equat. Meridian Degree.	56727·384	0 59 35·98 : 1 0 0 ·00	57108·40	....
		1 0 0·00 : 0 59 35 ·98	....	56348·88
Polar Meridian Degree.	57300·056	1 0 0·00 : 0 59 35 ·82	56915·18	....
		0 59 35·82 : 1 0 0 ·00	....	57687·52
" "	+572·67		−193·22	+1338·64
Ellipticity =	$+\frac{1}{99·06}$	The equatorial-meridian degree taken as unity.	$= -\frac{1}{294·56}$	$= +\frac{1}{43·07}$
			Mean = 572·71 = $+\frac{1}{99·06}$	

* These proportions are taken from the table, given by Professor Loomis in his "Introduction to Practical Astronomy" (New York,

This is a remarkable result, as exhibiting an extraordinary amount of confusion, contradiction, and error in the astronomical theory concerning the Earth's figure. In the first place there is the ellipticity of $\frac{1}{294 \cdot 56}$ to attract our attention. General Sabine remarks, in a note "On the Ellipticity of the Earth," appended to his translation of Von Humboldt's "Cosmos:" *—"Viewing the obvious tendency of the two methods—the pendulum and the measurement of degrees—to unite in one and the same conclusion, we may surely permit ourselves to regard the still subsisting very small difference between them as comparatively, if not wholly, insignificant. In regard, however, to this really slight remaining difference, I own that I am individually inclined *to give the preference to the pendulum result.* This preference may, no doubt, be in part owing to a natural bias in favour of what I have been myself engaged in, and, so far, be entitled to less weight; yet it is to be remembered that the pendulum result has the advantage of nearly ten degrees of greater meridional extension than the measured arcs, and that the latter have been limited almost exclusively

1855, 8vo.), pp. 374—377, in accordance with Professor Encke's results in the "Berliner Jahrbuch" for 1852, pp. 344—372. They differ but insensibly from those of Dr. Maedler's table.

* Vol. iv. pp. 483—484.

to the one (the northern) hemisphere, whilst pendulum experiments have been extended over both hemispheres with almost concurrent results." * The ellipticity just found, goes far to confirm this conclusion, as it indicates an error in the "radius vector" of Dr. Maedler's table, column vii., which gives an ellipticity of only $\frac{1}{299 \cdot 13}$, in accordance with the proportions generally assigned to the equatorial and the polar radius of the Earth.

Hence, even on the ground of the astronomical theory itself, not only the "Radii vectores" of the Earth, and the "Lengths of degrees of parallels," as computed by analysis and represented in Dr. Maedler's Table; † but, moreover, the whole of the theoretical conclusions, drawn by astronomy from the universally adopted ellipticity of the Earth of $\frac{1}{299}$, nearly, appear to be affected with corresponding errors.

This, however, is of little importance, when, in the second place, we find that such an ellipticity is altogether out of the question, and that the assumption of a polar *depression* of the Earth's shape *cannot possibly be correct.* For, whilst that assumption leads to an ellipticity of $-\frac{1}{294 \cdot 6}$, the contrary assumption

* "Pendulum Experiments" (1825), 4to., p. 346; "Note on the Ellipticity of the Earth," Cosmos, vol. iv., p. 458.

† See p. 25.

of a polar elongation—both assumptions, as such, being equally rational—leads to an ellipticity, not of the similar value of $+\frac{1}{295}$, but of a value nearly seven times more considerable, namely $+\frac{1}{43}$; the *mean* of which two values $= +\frac{1}{99}$, agreeing with the *empirical* result of geodetic measurements, and indicating, on the one hand, that the empirical result is the true one, and, on the other, that the *theoretical* result must necessarily rest on *some error of principle.*

And this error of principle is sufficiently apparent. *We have but to apply the very same rules, by which we have seen modern astronomy and analysis to convert a polar elongation into a polar depression, to the terrestrial spheroid of the astronomical theory, and we shall find them to* RECONVERT THE SUPPOSED POLAR DEPRESSION INTO A POLAR ELONGATION. For, in Fig. 18, let $H\ N\ E\ S$ represent the terrestrial spheroid, according to the astronomical theory. Then, *according to the same theory*, the lesser curvature at N belongs to the greater arc of 1°, $N\ A$, of a greater circle, of which B is the center, and $N\ B$ the radius; hence, the distance of N from the center (C') is $= N B$; but $N\ B = B\ C$, which, consequently, is the distance of $N = B$ from the centre $C = C'$, or the radius vector to the point $B = N$. Again, the greater curvature at E belongs to the lesser arc of 1°, $G\ E$, of a smaller circle, of which K is the center, and $K E$ the radius; hence, the distance of E from the center (C') is $= K E$; but $K E = C F$, which, consequently, is the distance of $E = F$ from the center $C = C'$, or the radius vector to the point $E = F$. Therefore, the Earth is not a spheroid, flattened at the poles, $H N E S$, as astronomy maintains; but, contrary to her theory, yet according to her own principles, a spheroid, $D B F I$, *elongated towards its poles*.

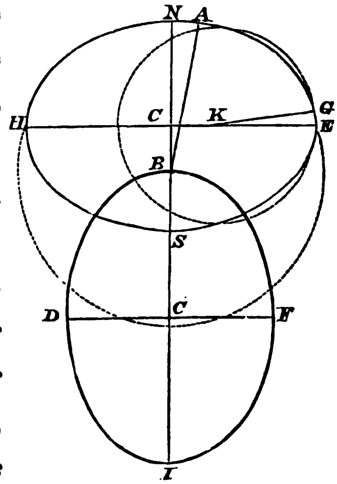


Fig. 18.

And this is fully borne out by Sir John Herschel's own argument. For, if the angles $G z E$ and $N x A$, Fig. 17, be each, as he states, "one (true) degree, *and, therefore, equal*," they must, as a matter of necessity, *refer to one common center*, the center of the Earth; and hence they establish the true proportions of our globe to be those of the direct empirical results of geodetic measurements, namely, 56727·384 toises for the equatorial-meridian arc $G E$, to which belongs the equatorial radius $E z$; and 57300·056 toises for the polar-meridian arc $N A$, to which belongs the polar radius $A x$, showing the polar diameter of the Earth to exceed its equatorial diameter in the proportion of 57300·056 : 56727·384, or by $\frac{1}{99}$ nearly.

Where, then, does the error of astronomy lie? It is easily traced. I take the following numbers from Professor Encke's tables:—

Geographical Latitude.		Angle of the Vertical.		Geocentric Latitude.			Geographical Latitude.		Angle of the Vertical.		Geocentric Latitude.		
°	′	°	″	°	′	″	°	′	°	″	°	′	″
0	0	0	0·00	0	0	0·00	90	0	0	0·00	90	0	0·00
	10	0	4·00	0	9	56·00	89	50	0	4·03	89	49	55·97
	20	0	8·01	0	19	51·99		40	0	8·06	89	39	51·94
	30	0	12·01	0	29	47·99		30	0	12·09	89	29	47·91
	40	0	16·02	0	39	43·98		20	0	16·12	89	19	43·88
	50	0	20·02	0	49	39·98		10	0	20·15	89	9	39·85
1	0	0	24·02	0	59	35·98	89	0	0	24·18	88	59	35·82
	10	0	28·02	1	9	31·98	88	50	0	28·21	88	49	31·79
......		......		......			......		......		......		
44	0	11	30·14	43	48	29·86	46	0	11	30·31	45	48	29·69
	10	11	30·29	43	58	29·71	45	50	11	30·42	45	38	29·58
	20	11	30·41	44	8	29·59		40	11	30·51	45	28	29·49
	30	11	30·50	44	18	29·50		30	11	30·58	45	18	29·42
	40	11	30·57	44	28	29·43		20	11	30·63	45	8	29·37
	50	11	30·62	44	38	29·38		10	11	30·65	44	58	29·35
45	0	11	30·65	44	48	29·35	45	0	11	30·65	44	48	29·35

From this it is seen, that astronomy has *chosen* for the proportions of reduction, in transforming the empirical polar elongation of the Earth into a theoretical polar depression, the differences for one degree between the "geographical" and the "geocentric" latitudes *at* 1° *and* 89° *of geographical latitude.* For what reason? A reason there is none. Any other two corresponding latitudes might have been selected for the same purpose with as much propriety, and in each case with different results. For, a degree is but a *conventional* part of a circle; and such a part would be equal to 3° 36′, were the circle divided into 100 degrees, or to 45° were it divided into 8 degrees; and the proportions of reduction, derived from each varying distance from the equator and the poles, will give a varying amount of ellipticity, or, which is the same thing, a varying proportion for the equatorial to the polar radius of the Earth. Hence, the source of the astronomical error is apparent. The proportion in question cannot possibly depend on the radius vector of the Earth either to 45°, 3° 36′, 1°, or to any other degree of latitude, except to 0° and 90°, *i.e., on the polar and the equatorial radii themselves;* and, therefore, the proportions for the reduction of the empirical length of a geographical meridian degree in 0° of latitude$=56727^{t}{\cdot}384$, to the theoretical length of a corresponding geocentric degree, and of the empirical length of a geographical meridian degree in 90° of latitude=57300^{t}·056, to the theoretical length of a corresponding geocentric degree, must necessarily be derived from the angular proportion of the geographical latitude to the geocentric latitude, *not in* 1° *and* 89°, any more than in 5° and 85°, *but in* 0° *and* 90° *of latitude.* Now, on referring to Dr. Maedler's or Professor Encke's Tables, we find, instead of 1° 0′ 0″·00 : 0° 59′ 35″·98 and 1° 0′ 0″·00 : 0° 59′ 35″·82, as erroneously taken by astronomy, this common proportion to be 0° 0′ 0″·00 : 0° 0′ 0″·00, *the angle of the vertical both in* 0° *and* 90° *of latitude being*=0. Hence, the empirical length of a geographical meridian degree in 0° of latitude =56727^{t}·384, is also the length of a *geocentric* meridian degree in 0° of latitude; and the empirical length of a geographical meridian degree in 90° of latitude,=57300^{t}·056, is also the length of a *geocentric* meridian degree in 90° of latitude: and hence, *the proportion of* 56727^{t}·384 : 57300^{t}·056 *is* (by Rule i.) *the true proportion of the equatorial-meridian to the polar-meridian radius of the Earth,* showing a polar elongation of $\frac{1}{99}$, nearly.

VII.

The preceding proof of the error of the astronomical theory, both in principle and in its application to the reduction of the empirical dimensions of the Earth to the proportions of its imagined shape, leave me still to consider it in connection with the determination of meridian degrees on the terrestrial surface.

In Fig. 19 let *C* mark the real center of the Earth; *e p C* the meridional section of a quadrant of its imagined figure, in harmony with the Newtonian theory; *s p′* an arc of one degree of a normal circle I, the center of which *C* is the Earth's center, and in which a star is supposed to occupy the geocentric position *s* at the distance of 1° from the north pole *p′*; *s″ p′* an arc of one degree of a similar circle II., of which *c* is the center. Now, the astronomical theory is this:—A plumb-line to the terrestrial pole *p* coincides with the radius *p′ C* of the circle I. When, however, on proceeding from the pole towards the equator

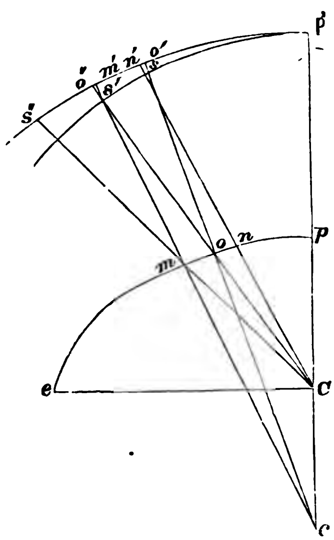


Fig. 19.

along the meridian $p\ e$, the declining direction of the plumb-line, prolonged upwards, passes through the star s, thus marking the celestial distance of 1° from the celestial pole p' in the circle I., this does not take place at the point n of the terrestrial surface, as would have been the case were all normals perpendicular to the Earth's center, but at the point o, because the direction of the plumb-line at o is perpendicular to a *tangent* to the position o, and, prolonged downwards, meets the polar axis of the Earth, not in its center C, but in a point c, as a center for the circle II. Hence, the "geographical" latitude of the position o = the arc $e\ o$, determined by the radius $o'\ c$ of the circle II., differs from its "geocentric" latitude = the arc $e\ n$, determined by the radius $s\ C$ of the circle I., by a distance on the terrestrial surface, equal to the arc $o\ n$, corresponding to the celestial arc $s'\ s$ in the circle I., because the radius vector of the Earth to o, prolonged, meets the circumference of that circle in s'. The angle $s'\ o\ s = c\ o\ C$ is termed "the angle of the vertical." It is assumed $= o\ C\ n$. And, therefore, it is argued, the arc $o\ p$ of one geographical degree, measured on the terrestrial surface, exceeding a geocentric or a true degree $= n\ p$, by the arc $o\ n$, its measured length has to be reduced in the proportion of $o\ p : n\ p$, in order to obtain the linear dimensions of a *true* polar meridian degree. A similar argument, in a reversed sense, and resulting in an inverse proportion, is applied to an equatorial-meridian degree.

Yet, plausible as these arguments may appear, they are, independently of their imaginary foundation, altogether fallacious of themselves. Supposing we wished to determine the proportions of some given *square* room, and were to measure one side of the room by a yard-measure and the other by a foot-measure; and supposing further, we were, without reducing both measures to one common standard of length, to apply them to the same angle of 90°, as projected upon paper, and to conclude the proportions of the room to be those of *the units found*, say 7 by 21: it is plain that, by neglecting one of the most elementary principles of geometry, we should from (assumed) correct measurements have drawn an utterly erroneous conclusion. This is the case of astronomy and analysis. They certainly apply the measures obtained of meridian arcs to one common center; but, disregarding that to the different radii p C and p c belong different circles and different arcs of degrees, $s\ p'$ and $s''\ p$, they fail to distinguish between the circle I., of which C is the centre of the Earth, and the circle II., of which c is the centre, *and to reduce the measures, taken in both circles, to one common circular measure.* For, in the circle II., the arc $s''\ p'$ has been assumed to measure *one degree,* corresponding to the similar arc *s p′* in the circle I.; and, therefore, the geocentric position of *o*, as determined by a plumb-line to it, falls in the circle II. *short* of one degree by the angle *s″* C *o″*, including on the terrestrial surface the arc *m o;* while in the circle I. it *exceeds* one degree by the angle *o″* C *n′*, including on the terrestrial surface the arc *o n*. But the angle *m′ c o′* being assumed = *o ″* C *n′*, we have the arc *m o* = *o n*. And the same argument applies, in an inverse sense, to an equatorial-meridian degree. Hence, the COMPLETE process of reducing the empirical length of a "geographical" meridian degree to the theoretical length of a "geocentric" or a true meridian degree, as adopted by astronomy and analysis,* should have been this :—

Latitude.	Length of a Geogr. degree.	Combined proportions of reduction.	Length of a true or Geocentric degree.
° ′ ″	Toises.	° ′ ′ : ° ′ ″	Toises.
0 0 0	56727·384	{ 0 59 35·98 : 1 0 0·00 1 0 0·00 : 0 59 35·98 }	56727·384
90 0 0	57300·056	{ 1 0 0·00 : 0 59 35·82 0 59 35·82 : 1 0 0·00 }	57300·056
		Polar meridian degree	+ 572·672
		Polar elongation	$\frac{1}{99 \cdot 06}$

from which it is seen, as I simply stated in the first edition of this Letter, that, "had astronomers not

* See pp. 54 and 60.

omitted that essential part of their geometrical process," to which I have just called attention, "they would, from two erroneous suppositions compensating each other, have arrived, themselves, at the true conclusion of the Earth's polar elongation."

It may possibly, however, be objected that my argument, based on a distinction between a circle of which p' C, and one of which p' c is the radius, does not hold good, because, as compared with the distance of the star s, the distance C c or the difference between the two radii p' C and p' c is a vanishing quantity. Exactly so. It is well that astronomy and analysis—the distinction being theirs, not mine—should remember, or be reminded of, this fact. For, what does their theory of "the angle of the vertical" really amount to? To nothing less than this: that, *with reference to the small linear extent of the terrestrial surface, which is comprised by the arc* o n, *the star* s *has a parallax equal to the angle* n′ s o′ = c s C = s′ o s, *or the angle of the vertical.* In other words, the theory in question implies that, as seen from the star s, the linear distance $o\ n$ on the Earth's surface subtends a sensible angle; and that this angle, being = 0 at the equator and at the poles, reaches in the latitude of 45° a maximum of 11′ 30″·65.

Thus, the astronomical theory of "the angle of the vertical" or of "the geographical *minus* the geocentric latitude" is found to suppose, *not only that the Earth has an indefinite number of centers of gravity; but, moreover, that, protean-like, its size is constantly changing*, in proportion as the arc subtended on the terrestrial surface by the angle of the vertical, and implied to vary in linear extent from 0 to at least 83,630,000,000 miles—if we take the distance of the star *s* at only 25 billions of miles—changes.

VIII.

From the accordant results, thus far arrived at, it would follow, that all plumb-lines or normals to the terrestrial surface meet in the center of the Earth; and, consequently, if we suppose a plumb-line, prolonged upwards, to pass through the star *s*, Fig. 19, page 63, and to determine f. i. the position of the Greenwich Observatory on the Earth's surface, that this position is not *o*, as astronomy theoretically teaches, but *n*, as she practically assumes. The next step in my argument, therefore, will be to show that, not only according to a true theory of gravity, but according to the Newtonian theory of gravitation itself, all plumb-lines *should* be perpendicular to the Earth's center.

True, Sir John Herschel* states:—"*It is an*

* "Outlines of Astronomy," p. 19.

observed fact, that in all situations, in every part of the Earth, the direction of a plumb-line is exactly perpendicular to still water; and moreover, that it is also exactly perpendicular to a line or surface, truly adjusted by a spirit-level." And such is the universal doctrine of modern astronomy. But, independently of numerous observed anomalies,* we possess no direct means of proving the strict truth of the asserted fact; for, to *every single point* in the meridional line of any given surface of still water, the direction of the plumb-line is a different one: that is to say, *there exists no such thing in nature as a perfect plane*, to which to refer, and by which to determine, the perfect perpendicularity of the plumb-line to such a plane. And even if we were to look upon a small surface of still water or any other fluid, contained within or by some narrow hollow or vessel, *as* a perfect plane, there would still remain the question, whether that plane is or is not, in every part of the Earth, at right angles with the terrestrial radius vector to it, and, consequently, whether all plumb-lines are or are not directed to the Earth's center; which again we have

* "Monthly Notices of the Astron. Society," vol. vii. p. 229; von Zach, in Lindenau und Bohnenberger's "Zeitschrift für Astronomie," Jahrg. 1818, p. 290; Airy, "Figure of the Earth," p. 236, &c.

no means of deciding by direct experiments. Such experiments even fail to show the perfect perpendicularity of the plumb-line to the plane, adjusted by the spirit-level.*

This explains, too, that Sir John Herschel, by way of proving his assertion, argues thus†:—"The [spirit-] level is a glass tube nearly filled with a liquid, the bubble in which, when the tube is placed horizontally, would rest indifferently in any part, if the tube could be mathematically straight. Suppose such a tube firmly fastened on a straight bar, and marked at two points distant by the length of the bubble; then, if the instrument be so placed that the bubble shall occupy this interval, *it is clear* that the straight bar can have no other than one definite declination to the *horizon;* because, were it ever so little moved one way or other, the bubble would shift its place and run towards the elevated side." Thus, the "empirical fact" is seen to rest on the mere assertion "it is clear;" but, as I have already remarked, it is by no means clear, what *possible* relation there can exist between the *horizon* of a geographical position and the position of the bubble in a spirit-level; and how the direction of

* Biot, "Traité Elém. d'Astronomie Physique," 3rd ed vol. i. p. 25.

† "Outlines of Astronomy," p. 105.

a mere *imagined* line can *possibly* determine the laws of natural phenomena. Moreover, it would be easy to prove, that the meridional line in the horizontal plane of any geographical position, excepting at the equator and at the poles, does *not* "exactly" coincide with a tangent to that position, whether we suppose the Earth to be an oblate or a prolate spheroid of revolution.

The astronomical doctrine concerning the direction of the plumb-line, therefore, as previously stated, is a mere assumption, had recourse to for the sole purpose of adapting the results of geodetic measurements to the imagined shape of the Earth of Sir Isaac Newton's theory, and resting neither on principle nor on fact.

I have elsewhere shown, that the phenomena of gravity, as relating to space and to the Earth, obey two distinct and utterly different laws; that the cause of one order of these phenomena is the repulsive force of space, or of that portion of condensed space, which encircles our globe, of the other the attractive force of the Earth; and that the latter force does not extend beyond the terrestrial surface. Sir Isaac Newton already distinguished between these laws, but, instead of consistently tracing them to their origin, following them out in their necessary consequences, and being led by them to a perception of the impossibility of the truth of his theory of universal attraction, *he blended them together, and thus introduced that dualism* into his system, which contains the germ of its certain destruction; which all the ingenuity and all the artifices, employed by himself and his disciples, have neither been able to veil nor to neutralize; and which leaves us only to wonder that the system should have endured so long.*

According to the theory, proposed by me, it is the hand of man or some other contrivance in immediate connection with the Earth, which supports the plumb-line against the repulsive force of space; and hence, since the direction of this force diverges from every point of the circumference of a wide sphere towards one common center,—the center of the Earth,—the direction of all plumb-lines or normals, dependent on it, is *of necessity* perpendicular to the Earth's center. But, on the surface of the terrestrial spheroid, the repulsive force of space being exactly equal to the expansive force of the Earth, because both are in a perfect state of equilibrium, and the latter force again being in the exact proportion of the extension of the Earth around its center, or in the direct proportion of the terrestrial radius vector: we also may, for argument's sake, consider the directions of the plumb-

* "Principia," Book i., Prop. lxxv.; comp. Book iii., Prop. ix.

line to depend on the attractive force of the Earth, as the conclusion, to which such an assumption leads, must, for the reason stated, be necessarily the same as it is in the actual case.

Now, if the Earth were a perfect sphere, it is universally admitted that all plumb-lines to the Earth's surface would then be perpendicular to the center of the Earth. Why? Not because, as astronomy teaches, they are perpendicular to the horizon or to a tangent to any point of the terrestrial surface, and, in the assumed case, a tangent and the horizon would form right angles with the Earth's radius: but because the direction of the plumb-line, dependent on the sum of the directions of the attracting particles, which constitute the Earth, must necessarily be the *mean* of those directions; and this mean direction passes through the center of the Earth. In other words, the direction of the plumb-line must necessarily be a neutral one with reference to the attracting particles, inasmuch as it represents the line of equilibrium of their attractive forces. Hence, the *plumb-line* may be defined as *a line, any plane laid through which, divides the Earth, as the attracting body, into two perfectly equal halves.* But as there is neither in a sphere, nor in any spheroid of revolution, whatever be the proportion of its axes, such a line that does *not* pass through the center of the body: it follows from the law of attraction itself, that all plumb-lines to the surface of the Earth, as a supposed spheroid of revolution, are perpendicular to its center.

The same may be shown from the 73rd Proposition of the 1st Book of Sir Isaac Newton's *Principia*, which reads thus:—"*If to the several points of a given sphere there tend equal centripetal forces decreasing in a duplicate ratio of the distances from the points; I say that a corpuscle, placed within the sphere, is attracted by a force proportional to its distance from the center.*

"In the sphere *A B C D*, Fig. 20, described about the center *S*, let there be placed the corpuscle *P;* and about the same center *S*, with the interval *S P*, conceive described an interior sphere *P E Q F*. It is plain (by Prop. LXX.) that the concentric spherical superficies, of which the difference *A EB F* of the spheres is composed, have no effect at all upon the body *P*, their attractions being destroyed by contrary attractions. There remains, therefore, only the attraction of the interior sphere *P E Q F*. And

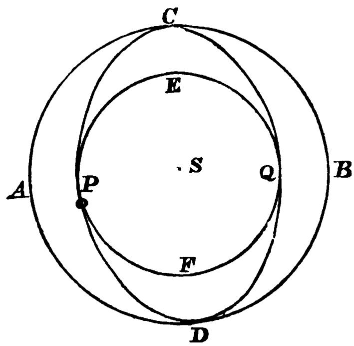


FIG. 20.

(by Prop. LXII.) this is as the distance *P S*.—Q. E. D."

This argument, if it be at all correct, applies with equal force to a plumb-line to the surface of a spheroid, conceived to be placed within the sphere *A B C D;* and hence, plumb-lines to all points of the terrestrial surface are perpendicular to the center of the Earth.

Again, Sir Isaac Newton states in the 75th Proposition of the 1st Book of the *Principia:*—"*If to the several points of a given sphere there tend equal centripetal forces decreasing in a duplicate ratio of the distances from the points; I say, that another similar sphere will be attracted by it with a force reciprocally proportioned to the square of the distance of the centers.* For the attraction of every particle is reciprocally as the square of its distance from the center of the attracting sphere (by Prop. 74), and is, therefore, *the same as if that whole attracting force issued from one single corpuscle placed in the center of this sphere;*" rom which it follows of necessity and would be easy to demonstrate, that all plumb-lines to the surface of a spheroid of revolution, as well as to the surface of a sphere, meet in the body's center.

Or, supposing we conceive within the meridional section of a spheroid of revolution a rectangle *a c e g* or an octagon *a b c d e f g h* Fig. 21, to be described:

it is generally admitted that plumb-lines to the points *a, b, c, d, e, f, g,* and *h,* will be directed to the center of gravity, and, therefore, meet in the center of either figure. And the same is true of a polygon of any number of sides. Hence, as we may consider the elliptic plane *h b d f* to form a polygon of an infinite number of sides, and as a spheroid of revolution is generated by the revolving of such an elliptic plane about either its major or its minor axis: the direction of the plumb-line to any point in the surface of a spheroid of revolution should, theoretically, be perpendicular to its center.

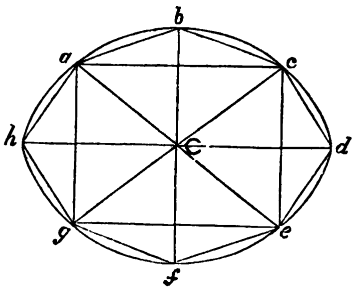


Fig. 21.

IX.

Of far greater importance, however, than the theoretical necessity, is *the empirical fact,* that all plumb-lines to the surface of the Earth *do* meet in its centre. This fact I will now proceed to prove.

Sir John Herschel states*:—"Defin. 16. The *zenith* and *nadir* of a spectator are the two points of the sphere of the heavens, vertically over his head

* "Outlines of Astronomy," pp. 61, 62.

and vertically under his feet, or the *poles* of the celestial horizon; that is to say, points 90° distant from every point in it." Suppose now, in Fig. 22, the spheroid *a b a′ b′* to represent the Earth, according to the present astronomical theory. Let a spectator be placed at *b*. Then, in the celestial sphere *H Z H′ N*, *H H′* will be his celestial horizon, coinciding with the extended plane of the terrestrial equator *a a′*; *Z* his zenith, coinciding with the celestial north pole *p*; *N* his nadir, coinciding with the celestial south pole *p′*; and *b′* his corresponding na-

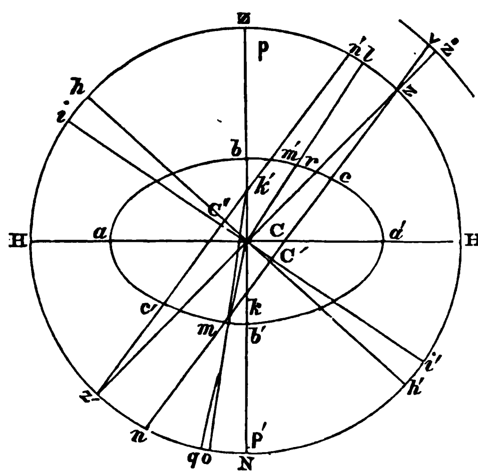


Fig. 22.

dir-point on the terrestrial surface. Each two of these points, viz., *b* and *b′* and *Z* and *N* will be 180° distant the one from the other, the former on the surface of the Earth, the latter in the celestial sphere; while each will be 90° distant, the latter from the celestial horizon, the former from the terrestrial equator—in perfect conformity with Sir John Herschel's definition.

Let the spectator now advance on the terrestrial meridian *b a′* from *b* to the position *c*, of which the geographical latitude shall be 45°, corresponding to a distance from the celestial north pole of 45°, because the direction of the plumb-line, in determining the assumed latitude, has declined 45° in the celestial sphere from $p = Z$ towards *H.′* Then, according to the astronomical theory, his celestial horizon will be *h h′;* his terrestrial horizon *i i′;* his zenith *z;* his nadir *n*, corresponding to the point *m* on the surface of the Earth. And similarly so in the southern hemisphere, on the spectator advancing from the south pole *b′* to the geographical position *c′* in the latitude of 45°. But, according to the same astronomical theory, the zenith of the geographical position *m*, which, as the nadir of *c* and as determined by the plumb-line to *c*, is in *n*, is, as determined by the plumb-line to *m*, in *o;* while its geocentric zenith is in *q*. And, as measured on the celestial sphere, the center of which is C, the zenith and nadir of the geographical position *c* are no longer 180° degrees distant from each other, but (taking the angle of the vertical C *z c* = 11′ 30″) 180° 23′ reckoned from west to east, and 179° 37′ reckoned from east to west. Nor is the distance of the zenith and nadir from the celestial horizon any longer 90°: but, while that of the zenith *z* continues to be so, the nadir *n* is from the eastern horizon distant only 89° 37′, from the western 90° 23′. While the direction of gravity at *b* is *b b′*, with the Earth's center of gravity in C, it is *c m* at *c*, with the center of gravity of the Earth in C′, and with that center in C″, *c′ m′* in *c′*. As seen from the star *z*, the small arc *r c* of the terrestrial surface subtends an angle of 11′ 30,″ which supposes that arc to measure upwards of 80,000 millions of miles; and, as seen from the equidistant star *Z*, the whole Earth appears a mere point, C, in space. Where there are such palpable incongruities and contradictions—contradictions involving concrete impossibilities,—there can be no truth.

It is an empirically-observed fact, that even the nearest star has no parallax with reference to the diameter of the Earth *a a′*, Fig. 22; and hence, that the whole Earth, at the distance of the star *Z* subtends no sensible angle, but represents a mere point in space without any apparent extension whatsoever. A plumb-line, therefore, perpendicular to the celestial horizon, whether it be conceived to be so at the terrestrial pole *b*, at the equator *a′* or at any intermediate station on the Earth's surface, *m′*, *r*, or *c*, *will, in all cases, point to the same star Z;* and whether a perpendicular be inclined to the celestial horizon at an angle of 45° at the pole *b*, the equator

a', or any intermediate point of the terrestrial surface, *it will in all cases point to the same star z*, provided that star itself be distant in the celestial sphere by an arc of 45° from the celestial pole *p* and the celestial horizon *H'*. Hence, two things are plain. Firstly, when a plumb-line is conceived to have an inclination of 44° 48′ 30″ to the celestial horizon *H H'*, forming with it the angle *p k v*=*p C l*, and being directed to the point *c* on the terrestrial surface, the geographical latitude, according to the astronomical theory, is 45°—either *the plumb-line cannot possibly point to the star z, the declination of which, Z z, in the celestial sphere is* 45°, *but must necessarily point to a star v, the declination of which, Z v, in the celestial sphere is* 44° 48′ 30″; or else, if it do point to the star *z*, as modern astronomy assumes it to do, that the star must be concluded to have a parallax of 11′ 30″, equal to the angle $v\,z\,z''=r\,z\,c$, with reference to the arc *r c* on the terrestrial surface, *and which it is known not to have.* In the second place, it is equally plain, that a plumb-line to the surface of the Earth, which *does* point to the star *z*, having a declination in the celestial sphere of 45°, *must of necessity form an angle of* 45° *with the celestial horizon H H'*, in consequence of the empirical fact stated; and hence, *that it must either coincide with, or be parallel to, the radius C z of the celestial sphere.*

But it is another empirically observed fact, that the plumb-line to the terrestrial surface, as we advance from the equator a' towards the pole b, *after coinciding with the celestial horizon* $H\ H'$ *at* a', *gradually assumes a position perpendicular to it, so as to coincide, at* b, *with the polar semi-axis of the celestial sphere* $Z\ C$.

Therefore, as to each single point in the celestial arc $Z\ H'$ of 90°, and to each single radius or right line, *joining those points and the center of the Earth*, being the center of the celestial sphere, there corresponds the direction of but one single plumb-line and but one single point in the arc $b\ a'$ of 90° of the Earth's surface: it follows as a necessary consequence, *that the directions of* ALL *plumb-lines or normals to the terrestrial surface coincide with corresponding radii of the celestial sphere, and, consequently, meet in the center of the Earth.*

Indeed, *wherever the figure of the Earth is not directly in question*, this is fully acknowledged by astronomy itself; and it is even insisted on, though in a somewhat confused manner, that the very same geographical latitudes, which are said to differ from the corresponding geocentric latitudes by the angle of the vertical, are, nevertheless, *determined by angles at the Earth's center*, and, consequently, by plumb-lines, meeting in the center of the Earth. Thus, Sir

John Herschel defines: *—"The *latitude* of a place on the Earth's surface is its angular distance from the equator, measured on its own terrestrial meridian. . . . Thus, the observatory at Greenwich is situated in 51° 28′ 40″ north latitude. This definition of latitude, it will be observed, is to be considered as only temporary. A more exact knowledge of the physical structure and figure of the Earth, and a better acquaintance with the *niceties* of astronomy, will render some modification of its terms, or a different manner of considering it, necessary." Somewhat further on he states: †—"The latitude of a station on a sphere would be merely the length of an arc of the meridian intercepted between the station and the nearest point of the equator, reduced into degrees. But as the Earth is elliptic, *this mode of conceiving latitudes becomes inapplicable*, and we are compelled to resort for our definition of latitude *to a generalization of that property* (art. 119), which affords the readiest means of determining it by observation, and WHICH HAS THE ADVANTAGE OF BEING INDEPENDENT OF THE FIGURE OF THE EARTH, which, after all, is not exactly an ellipsoid, or any known geometrical solid. *The latitude of a station, then, is the altitude of the elevated pole;* and is, therefore,

* "Outlines of Astronomy," p. 59.

† Ibid. p. 163.

astronomically determined by those methods already explained for ascertaining that important element." And the art. 119, to which Sir John Herschel refers, reads thus: *—"*The altitude of the elevated pole is equal to the latitude of the spectator's geographical station.* For it appears, Fig. 23, that the angle P A Z between the pole and the zenith is equal to N C A, and the angles Z A n and N C E being right angles, we have P A n = A C E. Now the former of these is e elevation of the pole as en from E, THE LATTER s THE ANGLE AT THE EARTH'S CENTER, subtended by the arc E A, or the latitude of the place."

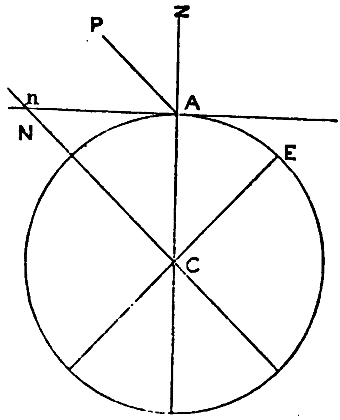


FIG. 23.

I will now, however, call your attention to a series special and numerous observations, contained in the *Astronomical Observations made at the Royal Observatory, Greenwich, under the direction of* GEORGE BIDDELL AIRY, *Esq., M.A., Astronomer Royal, published by order of the Board of Admiralty in obedience to Her Majesty's command.*" I have the volume for 1845 before me. The section "Observations of

* "Outlines of Astronomy," p. 71.

Zenith Distance with Troughton's Mural Circle, and Computations of Geocentric North Polar Distance," extends to 147 4to pages, and comprises about 3,500 observations. For the sake of illustration, I will quote of the printed details of a few of these observations, what is necessary for my purpose: —

Day.	Name of Object.	Apparent Zenith Distance.	Refraction.	Parallax.	Semi-diameter.	Geocentric N. P. D. of Center.
		° ′ ″	′ ″	′ ″	′ ″	° ′ ″
Jan. 27	γ Draconis	−0 1 48·03	0·03	...	...	38 29 33·74
Feb. 20	α Cygni	6 44 47·35	7·09	...	...	45 16 16·24
April 23	Polaris	−36 59 38·74	42·70	...	..	1 31 0·36
June 12	Collas Comet, S. P.	−83 41 16·39	8 3·60	9·95	...	−45 17 48·24 S.P.
„ 21	⊙ N. L.	27 44 56·69	29·48	3·90	15 45·30	66 32 29·37
	⊙ S. L.	28 16 28·59	30·14	3·97		66 32 31·26
„ 24	☾ N. L.	53 13 17·14	1 16·51	46 26·54	15 52·08	91 15 20·99
Aug. 25	☾ N. L.	31 7 14·38	34·68	27 52·26	14 48·04	69 26 6·64
„ 28	Mars, N. L.	71 30 11·12	2 53·60	21·31	13·36	110 4 18·57
„ 29	Venus, center ...	52 21 10·74	1 13·68	4·73	...	90 53 41·49
Sept. 1	Jupiter, N. L. ...	37 40 53·63	45·06	1·18	21·96	76 13 21·27
	Assumed Co-latitude 38° 31′ 21·80″.					

The apparent zenith distances of this table and of the Greenwich tables, generally, apply to the zenith of the Greenwich Observatory, *directly determined by the plumb-line to the Observatory*, as prolonged upwards to the sphere of the heavens. Adding refraction (and semi-diameter, when the northern limb of the object has been observed) to, and deducting parallax (and semi-diameter, when the southern limb has been observed) from, the "apparent" zenith distance, the "true" zenith distance of center is obtained; which, consequently, like the apparent zenith distance, *is directly determined by the plumb-line.* The "assumed co-latitude" of the Greenwich Observatory and the true zenith distance, *added together, give the "geocentric north polar distance of center.*" The geographical latitude of the Greenwich Observatory, according to the Nautical Almanac, is 51° 28′ 38″·2. The "assumed co-latitude," used in the Greenwich reductions, therefore, is the *geographical* co-latitude, which, like the true zenith distance, is directly determined by the plumb-line; so that BOTH THE ELEMENTS, OF WHICH THE "GEOCENTRIC" NORTH POLAR DISTANCES RE MADE UP, ARE IMMEDIATELY DETERMINED BY THE DIRECTION OF THE PLUMB-LINE TO THE OBSERVATORY.

Hence, the Greenwich "geocentric" north polar distances rest on the supposition of all plumb-lines, prolonged, passing through the center of the Earth;

and such being the case, it is plain, *either that the whole of those thousands and tens of thousands of places of the heavenly bodies, as reduced to geocentric north polar distance at the National Observatory* ARE ERRONEOUS; *or else, that they must be* ADMITTED *to prove*—what they do prove, namely—*that all plumb-lines to the terrestrial surface meet in the center of the Earth.*

Let, in Fig. 24,* C represent the center of the Earth and of the sphere of the heavens, p being the north-pole of the Earth, and NP that of the celestial sphere; p b the quadrant of a meridian, on which Greenwich shall be situated. Where? From the protean nature of modern astronomy, it is, in accordance with its theories, difficult to say. For, the dilemma, in which we are placed, is this:—supposing the star Z to be 45° distant on the celestial sphere from the north-pole, either Greenwich must be concluded to be situated *both* in F or H and in G; or else,

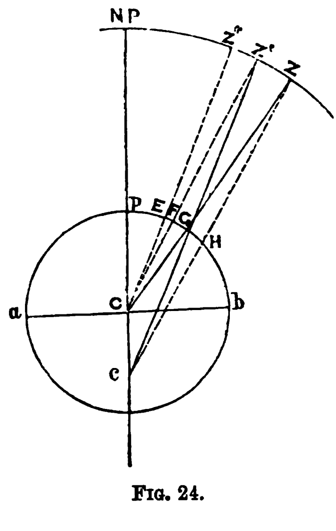


FIG. 24.

* Compare H. Breen (of the Royal Observatory), "Practical Astronomy," in "The Circle of the Sciences," vol. v.

assigning to Greenwich the position of G, the star Z must be *simultaneously* in Z′ and in Z. As the Royal Observatory, however, necessarily marks *some definite* point in its terrestrial meridian, let G be that point. Then, according to astronomy, Z is its geocentric, Z′ its "true" zenith, *directly determined by a plumb-line to* G. This "true" apparent zenith will be distant from the celestial north-pole N.P by an arc of 38° 31′ 21″·80, equal to the "assumed co-latitude" of the Greenwich Observatory; the geocentric (or the simply "true") zenith will be distant from the same pole of the heavens by an arc of 38° 42′ 35″·36, the "angle of the vertical" for the geographical latitude of the Observatory, = 51° 28′ 38″·20, being, according to Professor Encke's tables, −11′ 13″·56.

Now, on January 27, 1845, the star γ Draconis was observed at Greenwich, and its true zenith distance was found to be −0° 1′ 48″·06. But for this small angular quantity, its position would correspond exactly to the true zenith of Greenwich; and since this true zenith is said to differ by the angle of 11′ 13″·56 from the geocentric zenith: it is plain that the star's geocentric zenith should be 38° 42′ 35″·36 distant from the north-pole of the heavens; being the true zenith distance of Greenwich = 38° 31′ 21″·80 + the angle of the vertical. And, because the angle of the vertical for the small arc of 1′ 48″·06 is 0″·16:

it is equally plain that the geocentric north polar distance of the star γ Draconis, its true zenith distance being $-0° 1' 48''\cdot06$, should be $0° 11' 13''\cdot72 - 0° 1' 48''\cdot06 = +9' 25''\cdot66$. For, either there exists, as modern astronomy asserts, such a difference between the geocentric zenith and the true zenith, arising from the angle of the vertical, and then it has to be applied in the reduction of "true zenith distances" to *correct* "geocentric zenith," or "geocentric north polar distances;" or else, if the "true zenith distances," reduced to "geocentric north polar distances," exhibit no such difference, as is said to arise from the angle of the vertical, they show that, *unless all the reductions are acknowledged to be, and are, erroneous, such a difference does not exist at all.* But, as appears from our table, the reduction of the true apparent place of γ Draconis to its true geocentric place, *includes no element, answering to the angle of the vertical.* On the contrary, it supposes both places to be *identical* and, consequently, *the plumb-line to Greenwich to pass through the Earth's center.* And the same we find to be the case, not only with reference to α Cygni, Polaris, and the stars generally; but also with reference to comets and planets, the Sun and the Moon: in short with reference to *all heavenly bodies without exception.*

I might thus fairly leave the decision of this question to you. As my object, however, is to enable the reader to judge for himself, let him cast another glance at Fig. 24, in which, for argument's sake, the star γ Draconis shall in the first place be represented by Z. It will mark the geocentric zenith of Greenwich, G, and Z′ its true zenith. Then, the angle NP C Z will subtend a true arc in the heavens of 38° 29′ 33″·74, answering to the similar arc p G on the surface of the Earth, whatever be its exact shape. But a plumb-line to the National Observatory, G, is also found to meet, prolonged upwards, the star γ Draconis or the point Z in the heavens; being the true apparent zenith of G (in reality = 38° 31′ 21″·80, but here, for the purpose of illustration and greater simplicity's sake, assumed) = 38° 29′ 33″·74. Consequently, this plumb-line, being a right line coinciding with the terrestrial-celestial radius C G Z, *is perpendicular to the center of the Earth*, C; and cannot possibly coincide with the imagined "true zenithal" line c G Z′, nor be perpendicular to the imagined center c. On the contrary, a plumb-line to G, fulfilling the latter conditions, must necessarily point to the star Z′, distant from Z by the angle of the vertical Z G Z′ = 11′ 13″·56; and, therefore, the "true zenith" of G, of which the angle NP c Z subtends the geocentric north polar arc NP Z′ = 38° 29′ 33″·74 − 11′ 13″·56, ought to have been found = 38°

18′ 20″·18, instead of 38° 29′ 33″·74. Now, a plumb-line joining, prolonged, the star γ Draconis = Z and the imagined center *c*, will necessarily intersect the Earth's surface in H; and, since this point of intersection is the actual position of Greenwich, the National Observatory must be concluded to be situated "geocentrically" in G, and "true zenithally" in H, the linear distance between both stations, as resulting from the astronomical theory, being some 80,000 millions of miles.

Reversing the supposition, let now Z′ represent the star γ Draconis, in the assumed "true zenith" of Greenwich. Then, the geocentric angle NP CZ′ will subtend the celestial north polar arc NP Z′, = 38° 29′ 33″·74. But the "true zenithal" angle NP c Z′ corresponds to the geocentric angle NP C Z″ subtending the celestial north polar arc NP Z″, by the angle Z″ C Z′ = 11′ 13″·56 less than 38° 29′ 33″·74, or = 38° 18′ 20″·18. Consequently, a plumb-line passing through G to the imagined center c, cannot possibly, if the angle NP C Z′ = NP c Z subtend a celestial arc of 38° 29′ 33″·74, point to the star γ Draconis; but must necessarily point to a star Z″, by the angle Z″ C Z′ = Z′ c Z = 11′ 13″·56 nearer to the celestial north pole NP, or to a star, the north polar distance of which is 38° 18′ 20″·18. Hence, either the star γ Draconis must be concluded to be in Z, or Greenwich must be concluded to be situated in E; and *in both cases the plumb-line would be perpendicular to the center of the Earth.* Or, if the plumb-line to G *do* point to the star γ Draconis, 38° 29′ 33″·74 distant from the north pole of the heavens: then, the corresponding radius of the heavenly sphere will necessarily intersect the terrestrial surface in the point F; and, since this point of intersection is the actual position of Greenwich, the National Observatory must be concluded to be situated "geocentrically" in F, and "true zenithally" in G, the linear distance between both stations, as resulting from the astronomical theory, being again some 80,000 millions of miles.

If, then, it be simply admitted, on the one hand, that the stars occupy, apparently, a definite place on the celestial sphere, and that, with reference to the diameter of the Earth, they have no sensible parallax; on the other hand that, actually, the Earth is of a definite extent, and that the National Observatory occupies a definite site on its surface: it has also to be admitted that the Greenwich computations of geocentric north polar distances, *repudiating the astronomical doctrine of the angle of the vertical and of the deviation of the plumb-line,* are correct; and that the fact of all plumb-lines meeting in the center of the Earth, *is proved by thousands* *and tens of thousands of the most accurate astronomical observations.*

A few more examples may serve to illustrate this. The *declination* of a heavenly body is its geocentric altitude above the celestial horizon, and, consequently, the complement of the body's geocentric north polar distance to an arc of 90°. The sum of an object's declination and north polar distance, therefore, as represented by the true zenith distances of the object, directly and simultaneously determined by the plumb-line at two stations of different latitude, must necessarily differ from 90° by the difference of the angles of the vertical at those stations, if such an angle exists. On the contrary, if the sum be exactly or very nearly 90°, it supplies a positive proof that the directions of the plumb-line coincide with the corresponding geocentric radii of a great circle in the heavens, and, hence, meet in the center of the Earth, as being the center of that circle. Now, on the 21st of June, 1845, at Greenwich mean noon, the Sun's north declination, according to the Nautical Almanac, was 23° 27′ 28″·4; and, as it is an empirically ascertained fact—which of itself disproves the deviation of the plumb-line—that, on that day, a plumb-line to a station in 23° 27′ 28″·4 geographical north latitude points at noon to the center of the Sun, we may consider the given declination as directly determined, in the latitude in question, by the plumb-line. But, from our table, page 83, we find that, on the same day, the Sun's north polar distance, as also directly determined by the plumb-line to Greenwich from an observation of the Sun's southern limb, was 66° 32′ 31″·26. The sum of the Sun's declination and polar distance, as directly determined by the plumb-line on June 21, 1845, at two stations, the angle of the vertical at which is said to differ by 11′ 13″·56 — 8′ 23″·27 = 2′ 50″·29, is thus seen to be 66° 32′ 31″·26 + 23° 27′ 28″·4 = 89° 59′ 59″·7, or, within the fraction of a second in arc 90°, showing that the angle of the vertical has no existence.

Again, it appears from our table, page 83, that the Moon's apparent semi-diameter was, at some time on June 24-25, 1845, found to be 15′ 52″·08, from which we may conclude the time of observation. For, according to the Nautical Almanac, the apparent semi-diameter of the Moon, at midnight of June 24 = 15′ 55″·1, had, at noon of June 25, decreased to 15′ 47″·5. Hence, the observation must have been made at 4^h 46^m nearly, on the morning of June 25, or at 16^h 46^m June 24, astronomically speaking. At that time, we find from the Nautical Almanac, that the Moon's south declination was 1° 15′ 17″·3; that is to say, its center was vertical to 1° 15′ 17″·3 of south latitude, for which the angle of the vertical is said to be 30″·03. The north polar distance of the Moon, as directly determined by the plumb-line at Greenwich, and added to the Moon's south declination, similarly determined, should consequently differ from 90° by an arc corresponding to an angle of the vertical of 11′ 13″·56 + 30″·03 = 11′ 43″·59. But our table gives, for the time in question, the Moon's north polar distance, as directly determined by the plumb-line to Greenwich, to have been 91° 15′ 20″·99, which added to its south declination, as it would have been similarly determined in 1° 15′ 17″·3 of south latitude, = —1° 15′ 17″·3, gives 90° 0′ 3″·7, proving, once more, the direction of the plumb-line to the center of the Earth.

The same fact appears from the north polar distance, as directly resulting from the observed true zenith distance of the center of Venus on August 29, 1845, = 90° 53′ 41″·49; for, according to the Nautical Almanac, the computed places of which represent, mostly within a few seconds, the actual places of the planets, the center of Venus, at its transit over the meridian of Greenwich on August 29, 1845, had a south declination of —0° 53′ 43″·7, which, added to the north polar distance of 90° 53′ 41″·49, gives 89° 59′ 57″·8, or 90° within 2″·0.

I might greatly multiply these illustrations; but as there would be no use in so doing, I will only add some of a more striking character, taken from observations in *both* hemispheres, and in places situated nearly on the same meridian. Permit me, therefore, to call your further attention to the "*Zenith Distances observed with the Mural Circle at the Royal Observatory, Cape of Good Hope, and the Calculation of the South Polar Distances.*" I have the observations for the year 1837 before me. They are reduced precisely on the same principles as are those of Greenwich, the observed "true zenith distance," directly determined by the plumb-line, added to the geographical "assumed co-latitude," similarly determined, giving the "Geocentric South Polar Distances," which are, therefore, also directly determined by the plumb-line. The assumed co-latitude is 56° 3′ 56″·75; the geographical latitude of the Cape observatory, according to the Nautical Almanac, 33° 56′ 3″·[25] south; its longitude east of Greenwich 1^h 13^m 55^s. For the purpose of my present argument, therefore, we may consider the two Royal Observatories at Greenwich and at the Cape to be situated on the same meridian. Then the sum of the north and south polar distances of any given heavenly body, as directly and nearly simultaneously determined by the plumb-line to both Observatories, ought to differ from 180° by the sum of the two angles of the vertical, *i.e.*, by 11′ 13″·56 + 10′ 39″·09 = 21′ 52″·65,* provided such angles do exist. On the contrary, if that sum be exactly or nearly = 180°, it furnishes the proof that plumb-lines to the two Observatories, and, therefore, all plumb-lines generally, meet in the Earth's center.

Now, we find the geocentric polar distances, *i.e.*, *the true zenith distances, as directly determined by the plumb-line + the geographical co-latitude, also as directly determined by the plumb-line,* to have been observed as specified in the table, which occupies the following page.

The differences contained in the last column of this table further prove, and prove to evidence, that all plumb-lines are perpendicular to the center of the Earth; because, what are in astronomy termed—*for the reason just stated,* and for that reason alone, *truly* termed—the *geocentric* north polar distances of the heavenly bodies, *are made up of two elements, both directly determined by the plumb-line.*

* In order to perceive the necessity of this, the reader, bearing in mind that the polar distance of the true zenith corresponds to the geographical co-latitude, and its geocentric polar distance to the geocentric co-latitude, has but to compare the geographical latitudes of Greenwich and Capetown with the corresponding geocentric latitudes, as given in the table at page 25.

Date. 1837.	Object observed.	At the Cape. South.	At Greenwich. North.	Sum of Angles.	Difference from 180°.
		° ′ ″	° ′ ″	° ′ ″	″
Feb. 6	Jupiter...........	107 57 19·69	72 2 34·50	179 59 54·19	− 5·81
,, 13	Sirius	73 30 0·79	106 29 58·29	179 59 59·08	− 0·92
,, 20	Capella	135 49 41·77	44 10 18·94	180 0 0·71	+ 0·71
March 9/10	Sirius	73 29 59·94	106 29 59·84	179 59 59·78	− 0·22
,, 10	δ Canis Major...	63 51 33·47	116 8 31·42	180 0 4·89	+ 4·89
,, ,,	λ Ursæ Major ...	133 43 32·11	46 16 27·14	179 59 59·25	− 0·75
,, 14	ε Geminorum ..	115 17 14·18	64 42 45·88	180 0 0·06	+ 0·06
,, 20	β Virginis	92 40 50·03	87 19 8·65	179 59 58·68	− 1·32
April 7	Sirius	73 29 59·10	106 29 59·76	179 59 58·86	− 1·14
June 15	ε Scorpii	56 0 24·15	123 59 33·26	179 59 57·41	− 2·59
July 12	τ Herculis	136 42 21·58	43 17 40·96	180 0 2·54	+ 2·54
Aug. 25	ε Sagittarii	55 32 36·73	124 27 25·34	180 0 2·07	+ 2·07
Sept. 4	Uranus	80 1 58·66	99 58 3·73	180 0 2·39	+ 2·39
,, 5	Fomalhaut	59 31 10·98	120 28 47·33	179 59 58·31	− 1·69
,, 10	π Sagittarii	68 43 23·41	111 16 36·96	180 0 0·37	+ 0·37
Oct. 9	ι Aquarii	75 20 44·23	104 39 14·13	179 59 58·36	− 1·24

X.

Before drawing any deductions from the preceding results as to the true figure of the Earth, I will adduce two or three preliminary proofs of a more general nature, to show, both on the ground of the Newtonian theory and of empirical facts, that the Earth not only *should* be elongated at the poles, but also that it *is* so.

It is an admitted empirical fact, that gravity increases as we proceed from the equator towards the poles of the Earth. "The increase of weight," already Sir Isaac Newton observed,* "in passing from the equator to the poles is nearly as the versed sine of double the latitude;" and you yourself say,† "In going from the equator to the poles, gravity increases as the square of the sine of latitude." Now, "gravity," according to the Newtonian theory, is nothing save the attraction of the Earth; and since, according to the law of gravitation, the force of attraction is directly proportioned to the mass of the attracting particles,‡ it follows of necessity that, because terrestrial gravity is greatest at the poles and least at the equator, the number of attracting

* "Principia," book iii. prop. 20.

† "Figure of the Earth," p. 188.

‡ Sir John Herschel, "Outlines of Astronomy," p. 294.

particles of the Earth, which are in the direct proportion of their mass, must be least at the equator and greatest at the poles; and, consequently, that the polar radius of the Earth must exceed the equatorial radius, in the proportion of gravity at the poles to that at the equator. Indeed, it will be presently seen that, under the 20th Proposition of the 3rd Book of the "Principia," Sir Isaac Newton himself positively shows the Earth *to be elongated* towards its poles.

It is an observed fact that, in any latitude whatever, the mean annual temperature of a geographical station, and, on ascending higher at such a station, the temperature of the atmosphere decreases with the increasing distance from the center of the Earth. Thus, even in the torrid zones, the temperature of elevated positions is moderate, and, at certain altitudes, the Earth is found to be clothed in eternal snow and ice. Experience, therefore, teaches that, if we conceive a greatest sphere, contained within the terrestrial eroid, the greatest heat will be where the actual ace of the Earth coincides with the surface of sphere, and the greatest cold where the former ches the greatest elevation above the latter. ccording to the present astronomical theory, which teaches the Earth to be flattened at the poles, the torrid zones, consequently, should extend about the poles, and the frigid zone along the equator. But, empirically speaking, we find the reverse to be the case; and hence, *the very condition of the terrestrial surface shows, that the Earth, instead of being flattened, is elongated in the direction of its poles.*

It is an empirical fact, that the duration and the greatest phase of partial eclipses of the Moon, *calculated on the theory of a polar depression of the Earth of about* $\frac{1}{300}$, does not agree with the *observed* duration and magnitude; but that, in order to represent the latter more accurately, or, to use the words of Professor von Littrow,* "for the purpose of establishing a better accordance between theory and observation," the cone of the Earth's shadow or the value of $p + \pi + \mu$ has to be increased by about a $\frac{1}{60}$th part. In order to illustrate this, let, in Fig. 25, *a b c d* represent the Earth's shadow according to the present theory, *a e c f* its real shadow; M the Moon at its first

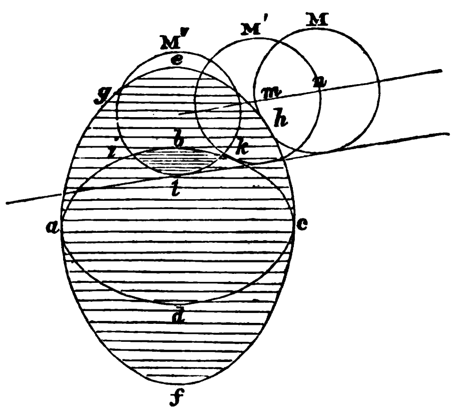


FIG. 25.

* "Theoret. und Prakt. Astronomie," vol. ii. p. 257.

contact with the real, M′ with the theoretical shadow; M″ at the maximum of obscuration. It is thus seen, by mere inspection, that the eclipse, in consequence of an erroneous theory concerning the Earth's figure, will commence too late by the time which the Moon requires to travel the distance *m n*, and end too early by a similar interval of time; the *duration* of the eclipse being, consequently, longer than it is calculated. And further it is seen, that the greatest phase of the eclipse, which obscures the portion *g e h l* of the lunar disk, will, according to the astronomical theory, obscure only the portion *i b k l* of the Moon's disk, and be observed by the quantity *e b* greater than it is computed. It is these *constantly varying* differences between the calculated and the observed phenomena and their times, which modern astronomy endeavours to remove—though, naturally, she succeeds but partially in so doing—by increasing the proportions of the Earth's shadow, that is to say, of the Earth itself by its $\frac{1}{60}$th part, without being able to assign a reason for such an augmentation.

M. Arago, certainly, considered it easy to explain away the difficulty by "conceiving that, under certain circumstances, certain atmospheric strata *may play the part of the solid and opaque portion of our globe* in the formation of the shadow, and that their thickness must be added to the radius of our solid globe;" * and on the strength of this dramatically absurd hypothesis, it is now frequently stated in dry words:—" Add the one-sixtieth part of $(P + p - \frac{D}{2})$ *for the effect of the Earth's atmosphere;"* † or " *on account of the terrestrial atmosphere* make $P = \frac{61}{60} (p + \pi + \mu)$": ‡ but, independently of every other consideration, it must have escaped M. Arago's attention, what he himself had stated in a preceding page, namely, "*that the height of our atmosphere is not above the* $\frac{1}{182}$*nd part of the Earth's semi-diameter.*" § With the Earth wrapped up in such an atmosphere as that conceived by the celebrated French astronomer, one might as well expect, by the flame of a rushlight, to see an eclipse of the Moon or any other phenomena of nature in the darkest cellar.

The differences in question, varying according to the direction of the Moon's path through the cone of the Earth's shadow with reference to its equatorial plane, and other elements, *furnish the cosmical proof of the Earth's polar elongation.* If, then, we take the expression $p + \pi + \mu$ at its mean value, and the

* "Popular Astronomy," vol. ii. p. 352.

† H. Breen, "Practical Astronomy," pp. 337, 338.

‡ Drechsler, " Die Sonnen-und-Mondfinsternisse," 1858, p. 176.

§ "Popular Astronomy," vol. ii. p. 120. The reader will not fail to perceive that M. Arago ascribes to the, "an opaque part acting" strata of the atmosphere, *twice the* "*thickness*" he ascribes to *the whole atmosphere itself.*

equatorial semi-diameter of the Earth, as seen from the Moon, at 15′ 32″ × 3·67; and if, in order to make the astronomical theory agree with observation, as regards the duration and the greatest phase of lunar obscurations, we have to write $\frac{81}{80}(p+\pi+\mu)$ for $p+\pi+\mu$: we find the value of that polar elongation to be about the $\frac{1}{100}$th part of the Earth's equatorial radius.

XI.

We may now revert to the empirical elements of Dr. Maedler's table,* that is to say, to the results of actual measurements of meridian degrees on the Earth's surface contained in the columns IV. and V. of that table.

In Fig. 26, let C mark the center of the Earth; ↠ the direction of plumb-lines to, and of the radii of, a great circle of the heavens passing through, the poles and the equator of a meridian plane of the Earth, and meeting in its center. Supposing now a to be an actual station

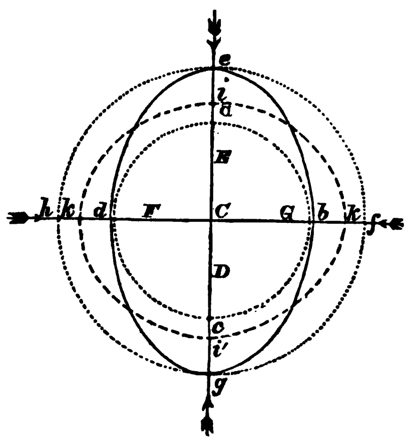


FIG. 26.

* See p. 25.

on the terrestrial surface, ascertained by observation to mark the very position of the north pole; and supposing, further, the Earth to be a perfect sphere. Then, it is generally admitted by astronomy and analysis, that *a C* would be the polar radius of the Earth, and, if the circle *a b c d* were described with this radius, that it would represent the circumference of the Earth; that the equatorial radius *b C* = *d C* would be equal to the polar radius *a C* = *c C*; and, if an arc of one degree at the equator *b* were found to measure 56727·384 toises, that *to that arc would belong the radius b C = d C.* And it is equally admitted, that if an arc of one degree, under similar suppositions, were found to measure, either at the equator or at the pole, 57300·056 toises, that *to such an arc would belong the corresponding radius e C*, exceeding in linear extent the radius *a C* in the proportion of 57300·056 : 56727·384; that the polar radius *e C* = *g C* would be equal to the equatorial radius *f C* = *h C*; and, if the circle *e f g h* were described with either, that it would represent the circumference of the Earth.

Now, it is as fully admitted by astronomy and analysis, whether the Earth be a spheroid of revolution revolving about its major axis, or a spheroid of revolution revolving about its minor axis, that plumb-lines to the two poles and to all points of the equator, coincide with the corresponding radii of a great circle of the heavens, the center of which is the Earth's center, and, consequently, *that they meet in the center of the Earth, precisely as though the Earth, so far as normals to those points are concerned, were a perfect sphere.*

Hence, whether in any spheroid of revolution whatever, a meridian arc of one degree be measured at the poles, or whether it be measured at the equator, *to such arcs belong*, by the admission of astronomy and analysis themselves, and in accordance with established principles of geometry, as well as with the analogy of observed facts, RADII TERMINATING IN THE CENTER OF THE EARTH. And hence, when it is found that a meridian arc of one degree, measured on the surface of the Earth at one of its poles, contains 57300·056 toises; and that a meridian arc of one degree, measured on the surface of the Earth at the equator, contains 56727·384 toises, *to such arcs* (being each one of 360 equal parts of two imaginary circles of different linear extent) *belong two corresponding radii of different linear extent*, BOTH TERMINATING IN THE CENTER OF THE EARTH, and, consequently, originating, the former at a distance from that center $=\frac{57300^{t}\cdot056\times360}{2\,\pi}=3{,}283{,}051$ toises, the latter at a distance from it $=\frac{56727^{t}\cdot384\times360}{2\,\pi}=3{,}250{,}231$ toises.

These results of actual geodetic measurements, therefore, empirically show the polar radius of the Earth to exceed its equatorial radius in the proportion of 3,283,051 : 3,250,231, or by the $\frac{1}{99 \cdot 03}$th part of the latter.

On the contrary, modern astronomy and analysis,—in defiance of their own rules and admissions, in defiance of every principle of geometry, in defiance of observed facts and concrete possibility,—make of the terrestrial radius, representing the actual extent of the terrestrial globe, *a sliding scale*, and of the one real center of the solid Earth *a shifting point*, for the sole purpose of adapting the given empirical proportions of the Earth, *d e b g*, Fig. 26, to the imagined proportions of the Earth of Sir Isaac Newton's theory, *i k i′ k′*. For, in the first place, *they shift the center of the Earth C*, as referred to a measured arc at the north pole *e*, *down to D*; and *down to E*, as referred to a measured arc at the south pole *g*: so as to obtain as *marks* for the originating points of the polar radius *e C*=*g C*, the points *i* and *i′* in the Earth's imagined surface; and to which marks thereupon, on applying the terminating points of the polar radii to the real center of the Earth *C*, their originating points *e* and *g are slided down*. On the other hand, *they shift the Earth's center up from C to F*, as referred to a measured meridian arc at the point *d* of the equator, and *up from C to G*, as referred to a measured meridian arc at the opposite point of the equator *b*: so as to obtain as *marks* for the originating points of the equatorial radius *d C*=*b C*, the points *k* and *k′* in the Earth's imagined surface, and to which marks, thereupon, on applying the terminating points of the equatorial radii to the Earth's real center *C*, their originating points *d* and *b are slided up*. And thus it is, that the true polar radius *e C* is converted into the theoretical polar radius *i C*, and the true equatorial radius *d C* into the theoretical equatorial radius *k C*; *i.e.*, that the true shape of the Earth, *d e b g*, is converted into the imaginary shape *k i k′ i′*.

Had not, in the modern system of theoretical astronomy, that protean, scrupulously-unscrupulous offspring of truth and error, of genius and perplexity, of fancy and confusion, "*Analysis*," usurped the rightful place of geometry, such sliding and shifting processes, as I have just had occasion to notice, could possibly have found no access into the noblest of human sciences; and Sir Isaac Newton's erroneous theory concerning the figure of the Earth must, long since, have given way before the irresistible force of subsequently-collected empirical facts. For, even for one moment admitting the results of those processes, namely:—

Geogr.-geocentric latitude.	Length of a meridian degree.	Length of a degree of the parallel.
0° 0′ 0″·00	56727^{t}·38	57108^{t}·40

it is plain, because the terms "a meridian degree in 0° of latitude," and "a degree of the parallel in 0° of latitude," the latter taken in a positive, the former in a theoretical sense, and having the same radius in common, are, as to extent, *synonymous expressions*, it is impossible for *both* the *differing* linear values ascribed to them to be correct. *Only one* OR *the other can be true.*

As I have already and fully shown,* the length of a meridian degree in 0° of latitude—a term which marks in a terrestrial meridian arc a mere point without any linear extension—is that of the $\frac{1}{360}$th part of a theoretical circle, equal to the real equatorial circumference of the Earth, *both circles being described with the same radius;* and hence the linear value of a meridian degree, and of a degree of the parallel in 0° of latitude—the latter being synonymous with a degree of the equator—are *in the strictest sense identical quantities.* Even, however, assuming the length of a meridian degree in 0° of latitude to mean the actual linear extent, measured

* See pp. 18—24.

on the Earth's surface, of a meridian degree, of which 0° degree of latitude or the equator marks the middle, that is to say, of a meridian degree, included by 0° 30′ of northern and 0° 30′ of southern latitude, this would produce a scarcely appreciable difference. Because, according to the calculations of astronomy itself,* the difference between a meridian degree in 0° and 0° 30′ of latitude is only

$$\begin{array}{rl} 0^\circ\ 0' = & 56727^{t}\cdot356 \\ 0^\circ\ 30' = & 56727\ \cdot399 \\ \hline = & 0^{t}\cdot043\ ; \end{array}$$

and, consequently, the *utmost* difference which can exist between an actual degree of the equator of the Earth and an actual meridian degree, included by 0° 30′ north and 0° 30′ south latitude, is $0^{t}\cdot086 = 6\cdot6$ English inches, or a little more than half a foot.

Indeed, *from the well-known proportions of the ellipse to the circle,* described about it with its major axis as a radius, it follows that we have but to multiply the linear dimensions of a meridian degree, in any given latitude, with the cosine of that latitude, in order to obtain the linear dimensions of a degree of the corresponding parallel; and since the cosine for 0° 0′ 0″·00 of latitude is 10·0000000, we find once more that the linear dimensions of a meridian degree

* Encke, im Berliner Jahrbuch für 1852, p. 345.

in 0° of latitude, and of an equatorial degree, *are necessarily identical quantities.* Astronomy and analysis, on the contrary, make out a difference between them of $-$ 381^{t}·136 = 2437 English feet for a *geographical* degree, according to Dr. Maedler, or of + 2563 feet for a *geocentric* degree according to Mr. Breen,* *implying a difference between the geographical and the geocentric meridian degree in* 0° *of latitude, of exactly* 2500 *feet as a mean.* Yet, the angle of the vertical in 0° of latitude, on which that difference depends, is = 0° 0′ 0″·00, the difference itself, consequently, being = 0 also; while the possible maximum of difference between a geocentric meridian degree in 0° of latitude and a geographical meridian degree, included between 0° 30′ north and 0° 30′ south latitude, as we have seen, scarcely exceeds six inches.

On the grounds, therefore, of arguments and facts like these, as well as on the strength of plain and unquestioned principles of geometry, which I imagined still to retain at least *some* weight with you, I was fully warranted in saying, as I did in the first edition of this Letter, that it would suffice to direct your attention to the *maximum* of difference between the computed lengths of degrees of parallels contained in the column VI. of Dr. Maedler's table,

* "Practical Astronomy," p. 214.

and their corresponding lengths, as deduced from the empirical lengths of meridian degrees, multiplied with the cosine of the latitude—a maximum reaching in 0° of latitude the value of 381·136 toises in one single degree, and showing, as it does, *that modern astronomy actually assigns two essentially differing values to the linear dimensions of the terrestrial equator*, namely, 56727·384 × 360 = 20,421,858 toises, and 57108·520 × 360 = 20,559,067 toises. For, "a degree of the parallel in 0° 0′ 0″ of latitude," I went on to say, "being identical with 'a degree of the equator,' and an arc of a degree of the equator and an arc of a meridian degree in 0° of latitude having the same radius in common, it follows (by Rule I.), that the linear dimensions of both degrees are the same (Rule X.). The only difference, indeed, between an actual degree of the parallel in 0° degree of latitude, and a theoretical meridian degree in 0° of latitude, consists in their being described, with the same radius, in different directions. Hence, if the former degree measures 57108·519 toises, the latter cannot measure less; and if the latter measures 56727·356 toises, the former cannot measure more."

"Which, then, of the two values ascribed by modern astronomy to an equatorial degree, is the true one? The greater value of 57108·519 toises is simply computed, on the theory of gravitation, to satisfy that theory; but without satisfying the few measurements of degrees of different parallels that have, thus far, been carried out. On the other hand, the lesser value of 56727·356 toises is the result of an actual measurement of the Earth's surface at the equator. So early as in the year 1735, Messrs. Bouguer and La Condamine were, by the French Government, sent out to Peru, to measure an equatorial meridian degree in that country, thence usually designated as 'the Peruvian arc.' It extended into both hemispheres, its middle being distant but half a degree from the equator. All the processes of the operation have been more than once investigated and recalculated, by eminent astronomers, with the utmost care; and the final conclusion, adopted by modern science, has been to assign to a meridian degree in 0° of latitude the linear value of very nearly 56727 toises. This therefore, is, approximatively at least, the *true* value both of a meridian degree in 0° of latitude, and of a degree of the equatorial circumference of the Earth."*

* The results of Bouguer and La Condamine's expedition virtually settle the entire question, as will appear from the sequel; but, as the problem involves, not only interests of vast magnitude, both material and scientific, but also great practical changes in the interior mechanism of Observatories, the office of the Nautical Almanac, and similar institutions, nothing short of a direct measurement of

For, when I had called your attention to "the acknowledged fact of all plumb-lines or normals to the terrestrial equator (supposed circular) and the two poles meeting in the Earth's center," I continued thus:—"Consequently the meridian arc about the equator, measured by Bouguer and La Condamine, was measured according to angular degrees, comprised between radii of a great circle, intersecting each other in the center, or, at any rate, within a few feet—an utterly vanishing proportion of the terrestrial radius—from the center of the Earth. And hence, the linear dimensions of 56727·384 toises, on the ground of that measurement assigned by astronomy to a meridian degree in 0° of latitude, are the linear dimensions both of a *true* meridian degree in 0° of latitude, and of a *true* equatorial degree, on the Earth's surface. And, for similar reasons, the linear dimensions of 57300·056 toises, assigned by astronomy to a meridian degree in 90° of latitude on the ground of the same and numerous other measurements of meridian degrees, must be taken to be the linear dimensions of a *true* meridian degree in 90° of latitude, described with its polar radius. Therefore (according to Rule I.), the empi-

a degree or of degrees of the equator ought to, and, in my opinion, will, satisfy either the public mind or the astronomical world. I myself am the very last person to desire it otherwise.

rical or actual proportion of the polar radius of the Earth to its equatorial radius is as 57300·056: 56727·384; showing a polar elongation of the Earth's true figure to the extent of nearly the $\frac{1}{99}$th part of its equatorial diameter." And this proportion once established, there followed from it, *as a necessary consequence*, that the linear dimensions of *all* meridian degrees, as given in Dr. Maedler's table, represent the linear dimensions of *true* angular degrees, that is to say, of angular degrees, *measured at the center of the Earth*, and, consequently, that all plumb-lines meet in the Earth's center.

My reason for not at once entering into a discussion of the astronomical theory of the angle of the vertical and of the deviation of the plumb-line, was a two-fold one. On the one hand, I desired to avoid all, what I conceived to be, unnecessary arguments; on the other hand, it was my wish to say, upon a subject which is so little calculated to reflect credit on astronomers and analysts, no more than the immediate aim, I had in view, appeared to me to demand. It is reluctantly I have seen myself compelled by yourself, to add to the strength of my other arguments those conclusive and "numerous proofs, including actual astronomical observations, which show that all plumb-lines or normals to the geometrical surface of the Earth, prolonged, intersect each other in the Earth's center."

These proofs, however, now given, must remove the last doubt—where such a doubt can still have existed—as to the fact of the identity of "geographical" and "geocentric" or true angular degrees, as directly determined on the Earth's surface by the plumb-line; and, consequently, of the linear dimensions of such degrees representing the *true* proportions of the terrestrial globe. But, on the ground of actual measurements of meridian degrees, included between 70° of north and 36° of south latitude, astronomy itself teaches the actual linear dimensions of a meridian arc of 90°, extending from the equator to the pole, to be .... = 5,131,179,811 toises.*

Computed with the linear value of a meridian degree in 0° 0′ 0″ of latitude, the same arc would measure 56727·356 × 90 = 5,105,462·040 „

e linear excess of an actual eridian arc of 90°, measured on the Earth's surface, ver an equatorial-meridian arc of 90°, consequently, is = 25,717·771 toises;

* Encke, Ueber die Dimensionen des Erdkörpers, nebst Tafeln nach Bessel's Bestimmungen, im Berl. Astron. Jahrbuch für 1852 (pp. 318—381), p. 381.

and thus we find once more (by Rule viii.) and from actual measurements of meridian arcs, that the Earth is elongated at the poles, and that the elongation of the polar diameter, calculated from Dr. Maedler's numbers, amounts to the $\frac{1}{99}$th part, nearly, of the equatorial diameter.

To render this still more plain. Let, in Fig. 27,

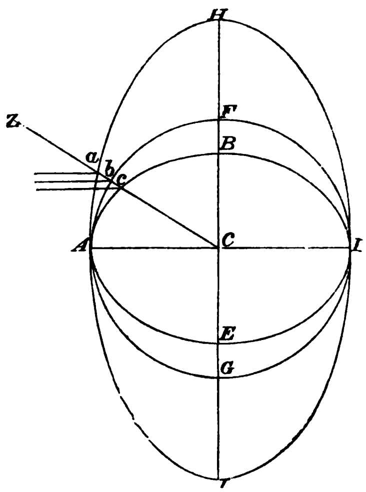


Fig. 27.

A D—the Earth's equatorial diameter.

A C—the Earth's equatorial radius.

Z C—a radius of the normal circle.

Z C A—an angle of one degree.

A b—a circular arc of one degree.

A a—an elliptical arc of one degree, greater than *A b*.

A c—an elliptical arc of one degree, less than *A b*.

A and *D* represent two actual positions on the Earth's surface, at the two extremities of its diameter, *A D*, in the plane of the equator. Then *A F D G*, the circle described with the equatorial radius *A C* = *C D*, will represent both the actual equatorial circumference of the Earth, and, assuming it to be a perfect sphere, its meridian circumference as well. Let *Z C* be the radius of a great normal circle, forming with the plane of the equator an angle of 1° at the Earth's center. Now, starting from the given point *A* towards the pole, assumed to be situated at *F*, we find, on following the same meridian, the linear distance on the actual surface of the Earth to the intersecting point in the radius *Z C*, to be *greater* than it would have been, if every point of the Earth's surface were at the same distance from its center as *A*, *i.e.* greater than the arc *A b*, described with the meridian radius of the Earth in 0° 0′ 0″ of latitude; the linear distance of the next degree greater again; and so on, the linear distance of every following degree greater than that of the preceding one up to the pole. Hence, the actual point, in which the radius *Z C* intersects the surface of the Earth, must necessarily be at a greater distance from *C* than *b* is; and every following point of intersection at a greater distance from *C* than the preceding one: because the sum of 90 linear distances of 1° each, progressing from the equator to the poles at a constantly increasing rate, is necessarily greater than the sum of 90 uniform distances, of a meridian degree at the equator each. Consequently, the entire meridian circumference of the Earth's real surface, too (*A H D I*), is greater than what the circumference would be, if the Earth, from the equator to the poles, were of the same dimensions as at the equator, *i.e.* greater than a meridian-equatorial circle

(*A F D G*). Therefore, assuming the Earth to be a spheroid of revolution, the meridian circumference of which would form an ellipse, in which the Earth's equatorial diameter (*A D*) is *given* as one of the axes, it follows of necessity that this is the minor axis of the ellipse; that, consequently, its major axis constitutes the Earth's polar diameter (*H I*); and hence, that the Earth is elongated at its poles. Such is, palpably, the only legitimate conclusion to be drawn from the empirical elements of Dr. Maedler's table, *i. e.* from the results of actual measurements of meridian arcs on the Earth's surface. That the polar elongation amounts approximately to the $\frac{1}{99}$th part of the equatorial radius, we have previously seen.

XII.

Having fully established that all plumb-lines to the geometrical surface of the terrestrial spheroid are perpendicular to the center of the Earth, and that the linear dimensions of a true equatorial arc of one degree are, not 57108·520 toises, as computed by modern astronomy, but (approximately at least) 56727·384 toises, as empirically determined by the French scientific expedition to Peru; it follows that all the "lengths of degrees of the parallel," contained in Dr. Maedler's table, column VI. (page 25), are erroneous, and cannot possibly agree with the results of actual measurements. *Nor do they.* It is true that astronomers, for this very reason, have regarded such measurements with an unfavourable eye, and chiefly confined their geodetic operations to meridian arcs: * but as these operations could not have been conveniently conducted without the aid of numerous sides of triangles, which represent small arcs of parallels, the means are thus afforded us of ascertaining, that the differences between the actual and the computed dimensions of degrees of parallels go on increasing from the poles towards the equator, until, at the equator itself, they reach the maximum of about 381 toises in one single degree.

The following table, calculated for every fifth degree of latitude, exhibits the differences of the linear dimensions of both longitudinal and latitudinal degrees, according to the present astronomical, and a true, theory.

* I perceive, however, from an abstract of your report to the Board of Visitors, in the *Parthenon* for June 21, 1862, that "Mr. Airy is devoting his attention to the completion of a great international undertaking—the measurement of an arc of parallel from the island of Valentia to the Volga." The present treatise will not fail to command the attention of the Governments, interested in that undertaking.

Latitude.	Length of a Degree of the Meridian in Toises.			Differences.		Length of a Degree of the Parallel in Toises.		Difference.	Radius Vector.	
	Present Theory.		True Theory.	True Theory.		Present Theory.	True Theory.	True Theory.	Present Theory.	True Theory.
	I.	II.		I.	II.					
0	57108·5	56727·4	56726·3	− 382·2	− 1·1	57108·5	56726·3	− 382·2	1·000000	1·000000
± 5	57107·1	56731·7	56730·9	− 376·2	− 0·8	56892·6	56515·0	− 377·6	0·999975	1·000081
10	57102·7	56744·5	56744·4	− 358·3	− 0·1	56246·6	55882·3	− 364·3	0·999899	1·000319
15	57095·8	56765·4	56766·5	− 329·3	+ 1·1	55174·9	54832·2	− 342·7	0.999778	1·000709
20	57086·3	56793·9	56796·5	− 289·8	+ 2·6	53685·4	53371·2	− 314·2	0·999612	1·001237
25	57074·7	56828·9	56833·4	− 241·3	+ 4·5	51788·8	51508·5	− 280·3	0·999407	1·001888
30	57061·1	56869·6	56876·2	− 184·9	+ 6·6	49498·7	49256·2	− 242·5	0·999170	1·002643
35	57046·1	56914·7	56923·4	− 122·7	+ 8·7	46832·0	46628·9	− 203·1	0·998907	1·003475
40	57030·0	56962·8	56973·8	− 56·2	+ 11·0	43808·1	43644·5	− 163·6	0·998626	1·004363
45	57013·5	57012·5	57025·6	+ 12·1	+ 13·1	40449·4	40323·2	− 126·2	0·998336	1·005277
50	58996·9	57062·3	57077·5	+ 80·6	+ 15·2	36780·7	36688·7	− 92·0	0·998045	1·006190
55	56980·8	57110·6	57127·7	+ 146·9	+ 17·1	3282·97	32767·1	− 62·6	0·997763	1·007076
60	56965·7	57156·0	57174·7	+ 209·0	+ 18·7	28626·0	28587·3	− 38·7	0·997499	1·007905
65	56952·0	57197·1	57217·2	+ 265·2	+ 20·1	24201·6	24181·6	− 20·0	0·997259	1·008654
70	56940·1	57232·6	57253·8	+ 313·7	+ 21·2	19590·1	19582·0	− 8·1	0·997052	1·009301
75	56930·6	57261·4	57283·6	+ 353·0	+ 22·2	14827·0	14826·1	− 0·9	0·996884	1·009825
80	56923·4	57282·7	57305·5	+ 382·1	+ 22.8	9949·0	9951·0	+ 2·0	0·996759	1·010211
85	56919·1	57295·7	57318·9	+ 399·8	+ 23·2	4994·0	4995·7	+ 1·7	0·996683	1·010447
90	56917·6	57300·1	57323·4	+ 405·8	+ 23·3	0·0	0·0	0·0	0·996657	1·010526
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.

This table is constructed on the supposition, that, a degree being one of 360 equal parts of a circle, there exist no various kinds of degrees as such. Astronomy and analysis, however, in opposition to this axiomatic truth, imagine "geographical" as distinguished from "geocentric" degrees, and assert that, if we regard the degrees of our first column as "geographical" degrees, they have to be diminished, but that they have to be increased, by the angle of the vertical, if we look upon them as "geocentric" degrees. In other words, *they suppose, —because the stars are known not to change their apparent places on the celestial sphere with reference to geographical positions on the Earth's surface,—that these "geographical" positions shift their places on the surface of the Earth, when viewed from its center, to the extent of the angle of the vertical, and thus turn into "geocentric" positions.* As the values of the columns II., VII., and X., however, are calculated on this supposition; that is to say, as they apply to latitudes, by the angle of the vertical less than those of column I., they will undergo certain modifications, on being computed for the tabular latitudes, regarded, in the astronomical sense, as "geocentric" latitudes. The following table will exhibit the comparative results in an instructive manner.

Latitude.	Radius Vector. Present Theory. I. Lat. Geogr.	Radius Vector. Present Theory. II. Lat. Geoc.	Radius Vector. Difference.	Length of a Meridian Degree in Toises. Present Theory. I. Lat. Geogr.	Length of a Meridian Degree in Toises. Present Theory. II. Lat. Geoc.	Length of a Meridian Degree in Toises. Difference.	Length of a Degree of the Parallel in Toises. Present Theory. I. Lat. Geogr.	Length of a Degree of the Parallel in Toises. Present Theory. II. Lat. Geoc.	Length of a Degree of the Parallel in Toises. Difference.
0°	1·000000	1·000000	0·000000	57108·5	57108·5	0·0	57108·5	57108·5	0·0
± 5	0·999975	0·999975	0·000000	57107·1	57107·1	0·0	56892·6	56889·8	2·8
10	0·999899	0·999898	0·000001	57102·7	57102·6	0·1	56246·6	56235·2	11·4
15	0·999778	0·999775	0·000003	57095·8	57095·7	0·1	55174·9	55150·2	24·7
20	0·999612	0·999607	0·000005	57086·3	57086·1	0·2	53685·4	53643·4	42·0
25	0·999407	0·999400	0·000007	57074·7	57074·3	0·4	51788·8	51726·6	62·2
30	0·999170	0·999161	0·000009	57061·1	57060·6	0·5	49498·7	49415·9	82·8
35	0·998907	0·998896	0·000011	57046·1	57045·5	0·6	46832·0	46728·9	103·1
40	0·998626	0·998615	0·000011	57030·0	57029·4	0·6	43808·1	43687·1	121·0
45	0·998336	0·998324	0·000012	57013·5	57012·8	0·7	40449·4	40314·1	135·3
50	0·998045	0·998034	0·000011	56996·9	56996·3	0·6	36780·7	36636·5	144·2
55	0·997763	0·997753	0·000010	56980·8	56980·2	0·6	32829·7	32682·5	147·2
60	0·997499	0·997490	0·000009	56965·7	56965·2	0·5	28626·0	28482·6	143·4
65	0·997259	0·997252	0·000007	56952·0	56951·6	0·4	24201·6	24068·8	132·8
70	0·997052	0·997047	0·000005	56940·1	56939·8	0·3	19590·1	19474·6	115·5
75	0·996884	0·996881	0·000003	56930·6	56930·4	0·2	14827·0	14734·7	92·3
80	0·996759	0·996758	0·000001	56923·4	56923·3	0.1	9949·0	9884·7	64·3
85	0·996683	0·996683	0·000000	56919·1	56919·0	0.1	4994·0	4960·8	33·2
90	0·996657	0·996657	0·000000	56917·6	56917·6	0·0	0·0	0·0	0·0
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.

These tables show, in a striking light, the incongruities, combined with the purely imaginary character of the present theory of the Earth's figure.

The values of column II. of the first table are calculated from the radii vectores of column X. and the theoretical length of an equatorial meridian degree = 57108t· 5. These values, multiplied with the cosine of the "geocentric" latitudes 4° 58′ 0″·5, 9° 56′ 4″·5, &c., as they appear in Dr. Maedler's table, page 25, and to which "geocentric" latitudes also the radii vectores appertain, give the lengths of degrees of parallels in column VII. They are repeated in the second table (I.), and compared with the corresponding values of the "geocentric" latitudes 5°, 10°, &c. (II.), computed from the astronomical theoretical elements. In the latter table, therefore, the columns "Theory I.," contain the values which modern astronomy ascribes to the "geographical" degrees, those of the columns "Theory II.," which she attributes to the "geocentric" degrees of the table. Hence, the latter represent, according to the present theory, the actual proportions of the Earth's dimensions, as calculated for every fifth degree of *true* latitude. *Neither of these values, nor those of column II. of the first table, are ever found in works on astronomy, whether of a scientific or of a popular character.* For the best of reasons not. A single glance at their figures renders the *utter* hollowness of the present theory too manifest.

In the first place, namely, we find from our comparative table that, as I have already shown by a different illustration, the very same principles, upon which astronomy has converted the empirical lesser dimensions of an equatorial, as compared with those of a polar degree, into theoretical greater dimensions, will, applied to the theoretical greater dimensions, reconvert them into the empirical lesser dimensions; and inversely so with reference to the dimensions of a polar degree.

In the second place, we perceive that, according to the astronomical theory itself, consistently applied, not only the lengths of an equatorial-meridian and an equatorial arc of one degree *are identical quantities*, but moreover, that the lengths of a "geocentric" and a "geographical" degree, whether of the meridian or of the parallel, in 0° and 90° of latitude, *are identical quantities as well*, having at each of those latitudes the same radius in common; and that, consequently, *if the geocentric degree at the equator measures* 57108^{t}·5, *the corresponding geographical degree cannot measure less; or, if the geographical degree at the equator measures* 56727^{t}·4, *the corresponding geocentric degree cannot measure more.* And similarly, if the geocentric degree at the poles measures $56917^{t}\cdot6$,* the corresponding geographical degree cannot measure more; or, if the geographical degree at the poles measures $57300\cdot^{t}1$, the corresponding geocentric degree cannot measure less. To render this quite plain, it will be as well to show that the values, assigned in columns V. and VI. of the comparative table, to the lengths of meridian degrees, *are truly those of the present astronomical theory*. This is easily done. For, since astronomy itself insists, that a *geocentric* meridian degree in 0° of latitude measures $57108^{t}\cdot5$, and, therefore, in accordance with a polar depression of nearly $\frac{1}{299}$, a *geocentric* polar degree $56917^{t}\cdot6$: the intervening values for a degree in any latitude ϕ, are easily calculated by the geometrical formula $\sqrt{(d \cos \phi)^2 + (d' \sin \phi)^2}$; and similarly so the corresponding radii vectores, the equatorial radius being taken as unity. But, for the corresponding *geographical* latitudes, we have both the radii vectores and the angle of the vertical given by astronomy itself; and, computing from both elements combined, the radii vectores for geocentric latitude, we find them to perfectly agree with the values pre-

* In accordance with the computed angles of the vertical, this length should have been found only $= 56915^{t}\cdot2$; whence the difference between the computed polar depression of $\frac{1}{299\cdot13}$ and $\frac{1}{304\cdot56}$, mentioned at page 55.

viously obtained. Thus we are enabled to calculate the lengths of meridian degrees for *geographical* latitude, which are those of column V.; and, on computing the same values from the lengths of degrees of parallels in column VIII., as again given by astronomy itself for geographical latitude, by dividing the latter with the cosine of latitude, we once more find the results to perfectly agree with those previously obtained. Hence, *the lengths of meridian degrees for geographical latitude, as given in column V. of our comparative table, page* 121, *represent their true values according to the present astronomical theory; being the values properly corresponding to the lengths of degrees of parallels for geographical latitude, as computed by astronomy and contained in column VIII.* If astronomers and analysts, then, after feeling in all meridian directions about the Earth's actual surface, have succeeded in discovering formulas of a purely empirical and tentative kind, which represent the linear dimensions of "geographical" meridian degrees to be 56727^{t}·4 at the equator and 57,300^{t}· at the poles, *this is in positive defiance of their own theory*, and constitutes but one of a thousand signs of the utter neglect and contempt, on the part of modern astronomy, of every geometrical principle, in these our days of "analytical triumph" and analytical *debauchery*.

In the third place, a glance at our tables teaches, that the essential difference between the "present" and "a true" theory of the figure of the Earth consists in this, that the latter is based on *fact*, the former on *fiction;* that the one correctly represents the *actual* dimensions of the *real* Earth, the other incorrectly the *fancied* dimensions of the *theoretical* Earth of Sir Isaac Newton's creative genius.

Lastly, our tables show, that the values in columns VI. and IX. of the first table represent the differences which, approximately, a comparison of the results of actual geodetic measurements with the dimensions, computed by the present methods, should exhibit; *and we shall presently find that such is the case.*

XIII.

Already Richer, who, in the year 1671, was sent by the French Government on a scientific mission to the island of Cayenne, and whose account of that mission was published several years before the first appearance of the "Principia" in 1687, had found from pendulum experiments, that gravity or "the attractive force of the Earth," was less near the equator than in higher latitudes; and there is little doubt but that Sir Isaac Newton's knowledge of this fact* had its share in introducing into his system that dualism to which I have referred before.† It strikes us most forcibly in the pendulum theory. While the author of the "Principia" remarks,—‡ "that there is a diminution of gravity, *occasioned by the diurnal motion of the Earth*, and therefore the Earth rises higher at the equator than it does at the poles, will appear by the experiments of pendulums related under the following proposition;" and under that proposition, *on the assumption of a polar depression of the Earth of* $\frac{1}{229}$, attempts to prove, "because the weights of the unequal legs of the canal of water *A C Q*, *q c a* (Fig. 10, page 29, note) are equal, and the weights of the parts, proportional to the whole legs and alike situated in them, are equal betwixt themselves," that, therefore, "the weights in all places round the whole surface of the Earth *are reciprocally as the distances of the places from the Earth's center:*" he gives a table of lengths of the

* "Principia," book iii., propos. 20.

† See page 71.—"Principia," book ii., propos. 24 :—

Cor. 1. If the *times* are equal, the quantities of matter in each of the bodies are *as the weights.*

Cor. 2. If the *weights* are equal, the quantities of matter will be *as the* SQUARE *of the times.*

By Defin. I. "quantity" and "weight" are synonymous expressions.

‡ Book III., propos. 19.

pendulum and of meridian degrees, computed for every fifth degree of latitude, according to which both go on *gradually increasing*—

		Feet.	Lines.		Toises.
from the equator or Lat.	0° =	3.	7·468	and	56637,
to the poles „	90° =	3.	9·387	„	57882;

in conformity with his Theorem "that the increase of weight in passing from the equator to the poles is nearly as the square at the sine of latitude." Now, nothing can be more plain than this argument; for, altogether independently of the circumstance, that Sir Isaac Newton's theory is unconscious of "the angle of the vertical" and "the deviation of the plumb-line," the proposition in question (Book iii. 20) states in distinct terms, though in contradiction with itself, that *the distance of the poles of the Earth from its center is to the distance of the equator from that center, as* 57382 : 56637, *or as* 441·387 : 439·468, which proportions give a polar *elongation* of the Earth, its equatorial radius being taken as unity, of $\frac{1}{129}$ from the lengths of the pendulum, of rather more than $\frac{1}{76}$ from the lengths of meridian degrees (being about $\frac{1}{110}$ as a mean).

The same amount of confusion and positive contradiction, in regard to the theory of the pendulum, prevails in the Newtonian school at the present day. On the one hand, with reference to the 73rd proposition of the 1st Book of the "Principia," it has to be acknowledged that the lengths of the seconds-pendulum are less within the Earth than at its surface, and that "at its surface they reach their maximum." * Hence, as these lengths are said to be directly proportional to the attraction of the Earth,† it follows that the minimum of attraction is at the Earth's center, where it becomes = 0; and that the lengths of the seconds-pendulum increase with the increasing matter of the Earth, or the increasing distance of its surface from the center. Therefore, since the empirical fact of the lengths of the seconds-pendulum and gravity increasing towards the poles cannot be denied,‡ the polar elongation of the Earth, in conformity with that element of the very law of universal gravitation,§ which makes attraction to be directly proportional to the mass or the number

* Maedler, "Popul. Astronomie," p. 84.

† Biot, "Traité Élém. d'Astronomie Physique," vol. iii. p. 393.

‡ See page 95.

§ In justice to the immortal author of the "Principia," it should be mentioned, that he is by no means accountable for the *modern conception* of "the law of universal gravitation;" and the worst service his admirers can render to his memory is, at the sacrifice of truth, to directly ascribe this law to him, with the view of showing that "Newton had thus ascended to the principle of gravitation in its most comprehensive form."—*Grant's History of Practical Astronomy*, p. 26.

Every human system must perish. So will that of Sir Isaac of attracting particles, results from it as an inevitable conclusion.

On the other hand, with reference to the 75th proposition of the 1st Book of the "Principia," astronomers *conceive* the attractive force of the Earth *to reside in its center;* and, unmindful of the circumstance that Sir Isaac Newton, under that same proposition, lets the attractive force of the Earth decrease outwards from the center in the proportion of *the square of the distance from the center,* they point to the 9th proposition of the 3rd Book of the "Principia," in which its author converts the quadratic proportion into a *simple* one from the Earth's surface inwards. Hence, *in direct contradiction with the former proposition and the law of gravitation itself,* from the lesser gravity at the equator its greater distance from the center of the Earth, as the *seat* of its attractive force, is concluded;* and thus the empirical fact of gravity increasing towards the poles is attempted to be reconciled with the Earth's assumed polar depression. But here, again, the contradictory element of the Newtonian theory concerning the pendulum, from which the

Newton. But *his* undying merit will remain for ever. It consists in having given to practical astronomy that impulse, the results of which alone will enable his posterity to erect a more extended and a more durable edifice on the ruins of his own.

* Maedler, "Popul. Astronomie," p. 74.

Earth's polar depression is said to follow, we find to be *a pure abstraction;* while the element which proves the Earth's polar elongation is *an empirical fact.*

There is no occasion for me to enter here into a critical discussion of the present erroneous theory of pendulum-motion. It will suffice to point to the observed fact, that the duration of the oscillations of the physical pendulum *is altogether independent of its substance and weight.** But as the weight of a body is usually understood to be nothing but an expression for the Earth's attraction upon it, there necessarily follows from the noticed fact, that the duration of the oscillations of the pendulum, and, consequently, these oscillations themselves, do not depend on the attraction of the Earth. Indeed, they virtually depend on the repulsive force of space. Let us assume the suspended pendulum to be at rest, that is to say, in a state of equilibrium between the force of space, endeavouring to repulse it towards the surface of the Earth, and the attractive force of the Earth, acting as resistance upon the pendulum as a lever, the fulcrum of which is its point of suspension. Then, on that equilibrium being disturbed by

* Newton, "Principia," book iii., præp. vi. Bessel, "Versuche über die Kraft, mit welcher die Erde Körper von verschiedener Beschaffenheit anzieht." Berlin, 1832.

a third force, the force of man, lifting the pendulum out of its state of equilibrium,* this motive force, though but once exercised and communicated to the pendulum, is, by the equal efforts of the two opposite forces to restore the equilibrium, converted into a perpetuated motion, which, under the given conditions, assumes the form of oscillations, confined within certain limits, depending on the repulsive force of space. The oscillations of the pendulum, therefore, may be considered as a function of the force of space, the velocity of each oscillation within the given limits alternately increasing and decreasing in a quadratic ratio.

This quadratic ratio is not only an observed fact, but, since the motion of the pendulum is the result of a force, decreasing as the *square* of the distance, it is *necessarily* of a quadratic nature. Hence, the present theory, according to which the length of the seconds-pendulum is directly proportional to gravity, rests on an erroneous application of known facts. The true law is, that *gravity is proportional to the*

* In the same sense Von Liebig remarks: "Our machines produce no force, but merely expend and apply whatever force may have been communicated to them. Thus the going of a clock is produced by a falling weight; but it is the force of the human arm that wound it up, which, through the motion of the wheels and of the pendulum, is expended again in 24 hours, in a week, or in a fortnight."—*Chemische Briefe*, 4th edit., vol. i., p. 205.

square of the length of the seconds-pendulum. And, therefore, the present theory, according to which gravity is proportional to the square of the number of oscillations made in equal times by the same pendulum, is likewise erroneous. The true law is, because observation has shown the times of oscillation to be as the square roots of the length of the seconds-pendulum, that *gravity is proportional to the fourth powers of the number of oscillations made in equal times by the same invariable pendulum.*

On the ground of these two laws we may, then, lay down the following general rule, with reference to the determination of the figure of the Earth from the phenomena of gravity:—At any given point of the (geometrical) surface of the Earth, the corresponding radius vector is proportional to the square of the length of the seconds-pendulum; or to the square of the velocity, with which free-falling bodies descend to the surface of the Earth in the first second of time; or to the fourth power of the number of vibrations, made in equal times by the same invariable pendulum.

By means of purely empirical formulas and tentative coefficients, astronomers have again succeeded in representing, by computation, the actual results of pendulum experiments with tolerable accuracy; but, *again, this accordance is only obtained* *at the sacrifice of every geometrical principle, and in diametrical opposition to their own theories.*

XIV.

I will now proceed to complete the whole argument, by indicating the leading features of a true theory of the Earth's figure, and showing it to be in perfect harmony with the results of all the most reliable measurements and observations, thus far carried out. Physical astronomy, as we have seen, has hitherto not succeeded in maturing such a theory, properly speaking; the theory established by Sir Isaac Newton being restricted to the imaginary *origin* of the imagined shape of our planet, while astronomy, during the intervening century and a half, has in vain sought for analytical formulas to establish, even by the aid of the most tentative coefficients, a tolerable harmony between the Newtonian theory and subsequently-collected facts. The following are outlines of a first attempt to reduce these facts *a theory of the Earth's figure*, requiring for its lication none but the most simple geometrical mulas.

After pointing out, what I believe to be the true theory of pendulum-motion, I have been enabled to lay down the following general law: *At* *any given geographical position on the surface of the Earth, the corresponding radius vector is proportional to the square of the length of the pendulum beating seconds; or to the square of the velocity with which free-falling bodies descend to the Earth's surface in the first second of time; or to the fourth power of the number of vibrations, obtained in equal times with the same invariable pendulum.* This law has now, in the first instance, to be applied to the determination, *independently of linear geodetic measurements,* of the proportion which the polar diameter of the Earth bears to its equatorial diameter, from the empirical elements of observed lengths of the seconds-pendulum; of observed velocities of free-falling bodies in the first second of time; and of observed numbers of vibrations of the invariable pendulum in a mean solar day: taking the numerical values of these elements such as they have been adopted by modern astronomy. The mean result I find to indicate a polar elongation of the Earth of almost exactly the $\frac{1}{98}$th part of its equatorial diameter; and which is in pretty close accordance with the corresponding value of $\frac{1}{99}$, deduced from the linear dimensions of meridian degrees, as (not quite correctly) determined by astronomers.

The simple process is this:—Arago,* on the ground

* "Astronomie Populaire," vol. iv., p. 69.

of the most careful and reliable pendulum experiments, states the probably correct lengths of the seconds-pendulum, reduced to vacuum and the level of the sea, to be

at the equator 991·027 millimètres
„ poles 996·189 „ ;

from which there would follow a proportion of the equatorial to the polar radius of the Earth

as $991{\cdot}027^2 =$ log. 5·9921709740 = 982134·516
to $996{\cdot}189^2 =$ „ 5·9966834838 = 992392·524

and, consequently, a polar elongation of $\frac{10258{\cdot}008}{982134{\cdot}516}$, or $\frac{1}{95{\cdot}74}$, the equatorial radius being taken as unity.

Again, Arago* states the velocity of free-falling bodies in the first second of time, as deduced from the most reliable observations, to be

at the equator 9·7803 millimètres
„ poles 9·8314 „ ;

which would give for the proportion of the equatorial to the polar radius of the Earth

$9{\cdot}7803^2 =$ log. 1·9806777096 = 95·648
to $9{\cdot}8314^2 =$ „ 1·9852307324 = 96·657

and, hence, for the polar elongation $\frac{1{\cdot}009}{95{\cdot}648}$, or $\frac{1}{94{\cdot}79}$.

* "Astronomie Populaire," vol. iv., p. 70.

In the "Encyclopædia Britannica,"* Mr. Galloway, the author of the article "Figure of the Earth," gives the observed numbers of oscillations, made by an invariable pendulum in a mean solar day, as follows:

Station.	Latitude.	Number of Oscillations.	Name of Observer.
	° ′ ″		
Rawak	0 1 34 S.	86261·46	Freycinet
Pulo Guansah Lout	0 1 49 N.	86266·64	Goldingham
....	....	....	....
Hammerfest	70 40 5 „	86461·14	Sabine
[Fort Bowen	73 13 39 „	86470·48	Foster]
Greenland	74 32 19 „	86470·72	Sabine
Spitzbergen.........	79 49 58 „	86483·28	Sabine

Hence the probable number of vibrations would appear to be

at the equator 86264
„ poles 86491;

and this gives for the proportion of the equatorial to the polar diameter

$$86264^4 = \log.\ 19{\cdot}7433183688 = 5537559029\ldots$$
$$86491^4 = \text{„}\ 19{\cdot}7478836736 = 5596076897\ldots$$

and, consequently, for the polar elongation $\frac{58517868}{5537559029}$, or $\frac{1}{94{\cdot}63}$.

The mean of these results indicates the polar

* Vol. ix., 8th ed., 1855, p. 574.

elongation of the Earth to be $\frac{1}{95 \cdot 05}$; and although this proportion may ultimately be found to differ from the true value in a trifling degree,—probably not by the $\frac{1}{10000}$th part of the equatorial radius,—we may adopt it at $\frac{1}{95}$ for all present purposes.

The second element we require to know, are the exact linear dimensions of an equatorial degree. As resulting from the measurements of Bouguer and La Condamine, they have been somewhat differently made out. The value given in Dr. Maedler's table is the result of Bessel's computation. Professor Encke takes it at the fraction of a toise less, namely, at $56727^{t} \cdot 356$. On the supposition of an ellipticity of $\frac{1}{95}$, I have, from the results of numerous geodetic operations and pendulum experiments deduced, as the more probably correct value, $56726^{t} \cdot 3$;* and since, under any circumstances, this fundamental element will finally have to be determined by further and direct measurements, we may here adopt the latter quantity.

On the basis of the two elements thus found, we are enabled to compute the linear values of the ter-

* "Kritisch-Populäre Briefe über die Neuere Astronomie," vol. i. The general preliminary results were given in the first edition of this Letter, at pp. 40, 41; which will account for the slight differences between them and the corresponding results, which appear in the present edition, as re-calculated with the corrected elements.

restrial radius vector for any given latitude, by the aid of the following geometrical formula, which rests on the proportion of the ellipse to the circle, described about it with its semi-major axis as a radius, viz.:—

$$r = \sqrt{\left[(d \cos \phi)^2 + (D \sin \phi)^2\right]};$$

and in which r is the radius vector; d the semi-minor, D the semi-major, axis of the Earth; ϕ the geographical latitude. The numerical results of such a calculation for every 30′ of latitude are embodied in the next table. The units of the computed number of toises are positively correct; and as the greatest error in the length of a meridian degree, which can arise from a neglect of the decimals, does not amount to half an inch, I have omitted them.*

* The following is the type of calculation :—

$d = 3{,}250{,}177\cdot171 =$	log.	6·5119070354	
Cos. 45° =	„	9·8494850021	
	log.	6·3613920375	
$(d \cos. \phi)^2 =$	„	12·7227840750	= 528182579....
$D = 3{,}284{,}389\cdot566 =$	log.	6·5164546643	
Sin. 45° =	„	9·8494850021	
	log.	6·3659396664	
$(D \cos. \phi)^2 =$	„	12·7318793328	= 539360791....
	log.	13·02838552	=1067543370....
$r = 3{,}267{,}328^{t}\cdot3 = \sqrt{\ } =$	„	6·51419276	

Latitude.	Radius Vector.	Differences.	Latitude.	Radius Vector.	Differences.
° ′	Toises.	Toises.	° ′	Toises.	Toises.
0 0	3,250,177	3	20 0	3,254,198	195
0 30	3,250,180	8	20 30	3,254,393	198
1 0	3,250,188	13	21 0	3,254,591	202
1 30	3,250,201	18	21 30	3,254,793	206
2 0	3,250,219	24	22 0	3,254,999	210
2 30	3,250,243	29	22 30	3,255,209	214
3 0	3,250,272	34	23 0	3,255.423	218
3 30	3,250,306	39	23 30	3,255,641	221
4 0	3,250,345	44	24 0	3,255,862	224
4 30	3,250,389	49	24 30	3,256,086	228
5 0	3,250,439	54	25 0	3,256,314	231
5 30	3,250,494	59	25 30	3,256,545	234
6 0	3,250,553	65	26 0	3,256,779	237
6 30	3,250,618	70	26 30	3,257,016	241
7 0	3,250,688	75	27 0	3,257,257	244
7 30	3,250,763	80	27 30	3,257,501	247
8 0	3,250,843	85	28 0	3,257,748	250
8 30	3,250,928	90	28 30	3,257,998	253
9 0	3,251,018	96	29 0	3,258,251	256
9 30	3,251,114	100	29 30	3,258,507	258
10 0	3,251,214	105	30 0	3,258,765	260
10 30	3,251,319	110	30 30	3,259,025	263
11 0	3,251,429	115	31 0	3,259,288	266
11 30	3,251,544	120	31 30	3,259,554	267
12 0	3,251,664	124	32 0	3,259 821	270
12 30	3,251,788	129	32 30	3,260,091	272
13 0	3,251,917	134	33 0	3,260,363	275
13 30	3,252,051	139	33 30	3,260,638	276
14 0	3,252,190	143	34 0	3,260,914	278
14 30	3,252,333	147	34 30	3,261,192	280
15 0	3,252,480	152	35 0	3,261,472	282
15 30	3,252,632	157	35 30	3,261,754	284
16 0	3,252,789	161	36 0	3,262,038	285
16 30	3,252,950	165	36 30	3,262,323	287
17 0	3,253,115	170	37 0	3,262,610	289
17 30	3,253,285	174	37 30	3,262,899	290
18 0	3,253,459	178	38 0	3,263,189	290
18 30	3,253,637	183	38 30	3,263,479	291
19 0	3,253,820	187	39 0	3,263,770	293
19 30	3,254,007	191	39 30	3,264,063	294

Latitude.	Radius Vector.	Differences.	Latitude.	Radius Vector.	Differences.
° ′	Toises.	Toises.	° ′	Toises.	Toises.
40 0	3,264,357	294	60 0	3,275,868	257
40 30	3,264,651	296	60 30	3,276,125	255
41 0	3,264,947	296	61 0	3,276,380	252
41 30	3,265,243	297	61 30	3,276,632	248
42 0	3,265,540	297	62 0	3,276,880	245
42 30	3,265,837	298	62 30	3,277,125	242
43 0	3,266,135	298	63 0	3,277,367	239
43 30	3,266,433	298	63 30	3,277,606	236
44 0	3,266,731	298	64 0	3,277,842	233
44 30	3,267,029	299	64 30	3,278,075	230
45 0	3,267,328	299	65 0	3,278,305	226
45 30	3,267,627	298	65 30	3,278,531	223
46 0	3,267,925	298	66 0	3,278,754	219
46 30	3,268,223	298	66 30	3,278,973	216
47 0	3,268,521	298	67 0	3,279,189	213
47 30	3,268,819	297	67 30	3,279,402	208
48 0	3,269,116	296	68 0	3,279,610	205
48 30	3,269,412	296	68 30	3,279,815	201
49 0	3,269,708	295	69 0	3,280,016	197
49 30	3,270,003	294	69 30	3,280,213	193
50 0	3,270,297	293	70 0	3,280,406	189
50 30	3,270,590	293	70 30	3,280,595	185
51 0	3,270,883	291	71 0	3,280,780	181
51 30	3,271,174	290	71 30	3,280,961	177
52 0	3,271,464	289	72 0	3,281,138	173
52 30	3,271,753	287	72 30	3,281,311	169
53 0	3,272,040	286	73 0	3,281,480	164
53 30	3,272,326	284	73 30	3,281,644	159
54 0	3,272,610	283	74 0	3,281,803	155
54 30	3,272,893	281	74 30	3,281,958	151
55 0	3,273,174	279	75 0	3,282,109	147
55 30	3,273,453	277	75 30	3.282,256	143
56 0	3,273 730	275	76 0	3,282,397	138
56 30	3,274,005	273	76 30	3,282,535	132
57 0	3.274,278	272	77 0	3,282,667	128
57 30	3,274,550	269	77 30	3,282,795	123
58 0	3,274,819	266	78 0	3,282,918	119
58 30	3,275,085	263	78 30	3,283,037	113
59 0	3,275,348	261	79 0	3,283,150	109
59 30	3,275,609	259	79 30	3,283,259	104

Latitude.	Radius Vector.	Differences.	Latitude.	Radius Vector.	Differences.
° ′	Toises.	Toises.	° ′	Toises.	Toises.
80 0	3,283,363	99	85 30	3,284,180	44
80 30	3,283,462	94	86 0	3,284,224	39
81 0	3,283,556	90	86 30	3,284,263	33
81 30	3,283,646	84	87 0	3,284,296	28
82 0	3,283,730	79	87 30	3,284,324	24
82 30	3,283,809	75	88 0	3,284,348	18
83 0	3,283,884	70	88 30	3,284,366	13
83 30	3,283,954	64	89 0	3,284,379	8
84 0	3,284,018	59	89 30	3,284,387	3
84 30	3,284,077	54	90 0	3,284,390	
85 0	3,284,131	49			

For the purpose of computing from this table the theoretical lengths of meridian degrees, of degrees of parallels, and of the seconds-pendulum, as well as the number of vibrations of the invariable pendulum, for any given geographical position, I use the following formulas.

To calculate the linear dimensions of meridian degrees, I substitute for the tentative formula of astronomers *

$$57013{\cdot}109^{t} - 286{,}337 \cos 2\phi + 0{\cdot}611 \cos 4\phi - 0{\cdot}001 \cos 6\phi,$$

the purely theoretical formula

$$r . \frac{\pi}{180} .$$

To calculate the linear dimensions of degrees of

* Maedler, "Popul. Astronomie" (1861), p. 23.

parallels, I substitute for the tentative astronomical formula *

$57156{\cdot}285^t \cos\phi - 47825 \cos 3\phi \times 0{\cdot}060 \cos 5\phi$,

the purely theoretical formula

$$r \cos\phi . \tfrac{\pi}{180} .$$

To compute the lengths of the seconds-pendulum, I substitute for the astronomical formula †

$$39{\cdot}01677 + {\cdot}20277 \times \sin^2\lambda - {\cdot}00880 \times \sin^2\lambda . \cos^2\lambda,$$

which is of a purely tentative nature, the purely theoretical formula

$$\sqrt{\left(\tfrac{r}{R} . p^2\right)}.$$

In the former, which you yourself propose as the best formula you are able to offer,‡ λ represents the geo-

* Maedler, "Popul. Astronomie," p. 23.

† Airy, "Figure of the Earth," p. 231.

‡ Your own words are :—"It is impossible to avoid remarking that in the high north latitudes the greater number of errors have the sign +, and that about the latitude 45° they have the sign — : those about the equator being nearly balanced. No alteration of our numbers is sufficient to correct this. If we increased the multiplier of $\sin^2\lambda$, we might make the errors at high latitudes as nearly balanced as those at the equator : but then those about latitude 45° would be still greater than at present. To destroy these without altering the others we may add a term $- A. \sin^2\lambda . \cos^2\lambda$. We have seen that such a term ought to exist, though we could only offer a conjecture as to its magnitude. From the table of errors it would seem that the length of the seconds-pendulum would be better expressed by $39{\cdot}01677 + {\cdot}20277 + \sin^2\lambda - {\cdot}00880 \times \sin^2\lambda . \cos^2\lambda$. The last term is ten times as great as our imperfect theory would

graphical latitude; in the latter, r the radius vector for that latitude; R the equatorial radius; p the empirical equatorial length of the seconds-pendulum.

And, in a similar manner, to compute the number of vibrations of the invariable or the normal pendulum in a mean solar day, I substitute for the astronomical, purely tentative formula *

$$86246{\cdot}8\ (1 + {\cdot}00514491 \sin^2 l)^{\frac{1}{2}}$$

the purely theoretical formula,

$$\sqrt[4]{\left(\frac{r}{R} \cdot s^4\right)},$$

in which s represents the empirical number of vibrations at the equator.

lead us to expect. Still, as an empirical formula, this is the best that we can offer."

You subsequently add, in reference to the determination of the Earth's ellipticity: "The question now is, what value shall we *choose* for F? Shall we represent the length of the seconds-pendulum by $39{\cdot}01677 + {\cdot}20027 \sin^2 \lambda$, or by $39{\cdot}01677 + {\cdot}20277 \sin^2 \lambda - {\cdot}00880 \sin^2 \lambda \cos^2 \lambda$? The former gives $F = {\cdot}005133$; the latter gives $F = {\cdot}005197$. We are now in a case where our theory partly fails, and where it is impossible to say which of the empirical forms is *more likely* to be correct. If we adopt the former value of F we find $e = {\cdot}003535$; if the latter, $e = {\cdot}003471$. Either of these values of e is considerably greater than that which we found from the discussion of the geodetic measures (·003352). We cannot offer any conjecture as to the cause of this difference."—*Airy, Figure of the Earth*, p. 231.

* Galloway, Art. "Figure of the Earth," in "Encycl. Brit." vol. ix. (1855), p. 574.

The types of calculation are these. Adopting for the equatorial values of a meridian degree 56,726·3 toises; of the length of the pendulum beating seconds 39·01350 English inches; and of the number of vibrations of the invariable pendulum in a mean solar day 86261,—being the most probably correct quantities, as deduced by me from a great number of empirical measurements and experiments,—I form the following constants:—

for meridian degrees and degrees of parallels—

$$\begin{array}{rcll} \pi & = & \text{log.} & 0{\cdot}4971499 \\ 180 & = & \text{,,} & 2{\cdot}2552725 \\ \hline \frac{\pi}{180} & = & \text{log.} & \bar{2}{\cdot}2418774\,; \end{array}$$

for the seconds-pendulum—

$$\begin{array}{rcll} p = \overset{\text{inches}}{39{\cdot}01350} & = & \text{log.} & 1{\cdot}5912149 \\ \hline p^2 & = & \text{,,} & 3{\cdot}1824298 \\ R = 3{,}250{,}177^{t}{\cdot}171 & = & \text{,,} & 6{\cdot}5119070 \\ \hline \frac{p^2}{R} & = & \text{log.} & \bar{4}{\cdot}6705228\,; \end{array}$$

for the invariable pendulum—

$$\begin{array}{rcll} s = 86{,}261 & = & \text{log.} & 4{\cdot}9358145 \\ \hline s^4 & = & \text{,,} & 19{\cdot}7432578 \\ R & = & \text{,,} & 6{\cdot}5119070 \\ \hline \frac{s^4}{R} & = & \text{log.} & 13{\cdot}2313508 \end{array}$$

Then, for the latitude of 45°, for instance, we have—

r (by table)	$= 3,267,328^{t.}$	= log.	6·5141927
	$\frac{\pi}{180}$	= „	$\bar{2}$·2418774
Length of a meridian degree	$= 57,025^{t.}6$	= log.	4·7560701
	Cos. 45°	= „	9·8494850
„ „ a degree of the parallel	$= 40,323^{t.}2$	= log.	4·6055551
	r	= log.	6·5141927
	$\frac{p^2}{R}$	= „	$\bar{4}$·6705228
		log.	3·1847155
„ „ the seconds-pendulum	= 39·11630 inches = $\sqrt{}$	= log.	1·5923578
	r	= log.	6·5141927
	$\frac{s^4}{R}$	= „	13·2313508
		log.	19·7455435
Number of vibrations of the invariable pendulum	$= 86,374·6 = \sqrt[4]{}$	= log.	4·9363859

Is Delambre correct in stating that "tous les calculateurs aiment surtout ce qui est *facile, court et commode, et* PAR UNE SUITE NÉCESSAIRE, PLUS EXACT?" * If so, these formulas have some claim to the gratitude of calculators; for, at present, it is "*hodman's work,* which consumes the time of the astronomer." †

* "Histoire de l'Astronomie au XVIII. Siècle," p. 352.

† Nichol, "The Planet Neptune," p. 68.

XV.

Our first attention has now to be directed to *meridian* degrees. Foremost among the great geodetic operations, which have been undertaken in modern times, ranks the measurement of a meridian arc of upwards of 25°, included between 70° 40′ 11″·3 and 45° 20′ 2″·8 north latitude, performed by order of the Russian Government, by and under the chief direction of William von Struve, one of the most eminent astronomers and geodetists of our age. It is almost impossible to speak too highly of the manner in, and the consistency with, which this vast undertaking has been carried out. It is a noble monument of human science and enterprise, reflecting equal honour on the Government who called it into life, the distinguished men who conducted it, and every one engaged in its execution. So far as the results, as a whole, are known, they have been communicated by General von Schubert, in the "Astronomische Nachrichten," whence I have taken the particulars embodied in the following table, as compared with the corresponding results, obtained by computation, on the new theory, and with the formula just given.

RUSSIAN ARCS.

No.	Stations.	Observed Latitudes.	Amplitude of Arc.	Mean Latitude.
		° ′ ″	° ′ ″	° ′ ″
	Fuglenaes	70 40 11·3		
1			1 59 12·9	69 40 34·9
	Stor-Oiwi	68 40 58·4		
2			2 51 13·7	67 15 21·5
	Torneå	65 49 44·7		
3			3 11 39·7	64 13 54·9
	Kilpi-Maeggi......	62 38 5·0		
4			2 32 54·95	61 21 37·5
	Hochland	60 5 10·05		
5			1 42 22·45	59 13 58·8
	Dorpat	58 22 47·6		
6			1 52 42 8	57 26 26·2
	Jacobstadt	56 30 4·8		
7			1 50 58·9	55 34 35·8
	Niemèz	54 39 5·9		
8			2 36 23·7	53 20 54·0
	Belin	52 2 42·2		
9			1 56 52·2	51 4 16·1
	Kremenec	50 5 50·0		
10			1 20 46·9	49 25 26·6
	Suprunkofcy	48 45 3·1		
11			1 43 37·9	47 53 14·1
	Wodolui	47 1 25·2		
12			1 41 22·4	46 10 44·0
	Staronekrasofka...	45 20 2·8		

The radius vector (column VII.) for the middle latitude (column V.) of the measured arc (column IV.), is taken from the table, pp. 140-142, and from it the theoretical length of a degree (column X.) for the given mean latitude is computed; while the principal factors in column IX. represent the measured length of a degree for the same latitude, as deduced from the astronomical amplitude of the arc (column

RUSSIAN ARCS.					
No.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differ-ence.
			Measured.	Computed.	
	Toises.	Toises.	Toises.	Toises.	Toises.
1	3,280,281	113,753·9	57,251·5 + 9·2	57,251·7	+ 9·0
2	3,279,297	163,221·9	57,207·6 + 8·9	57,234·5	− 18·2
3	3,277,950	182,794·3	57,223·9 + 8·5	57,211·0	+ 21·2
4	3,276,562	145,713·6	57,174·2 + 8·0	57,186·8	− 4·6
5	3,275,470	97,538·6	57,166·0 + 7·7	57,167·7	+ 6·0
6	3,274,518	107,280·6	57,108·1 + 7·4	57,151·1	− 35·6
7	3,273,496	105,730·9	57,161·3 + 7·1	57,133·3	+ 35·1
8	3,272,240	148,809·5	57,089·9 + 6·7	57,111·4	− 14·8
9	3,270,925	111,219·0	57,098·8 + 6·3	57,088·4	+ 16·7
10	3,269,959	76,751·4	57,006·5 + 6·0	57,071·5	− 59·0
11	3,269,050	98,558·0	57,062·5 + 5·7	57,055·7	+ 12·5
12	3,268,032	96,415·1	57,065·4 + 5·4	57,037·9	+ 32·9

IV.) and its measured geodetic length (column VIII.) The small additive quantities alone (in column IX.) require an explanation.

It must be remembered by the general reader, that the measurement of the linear extent of degrees on the Earth's surface is effected by triangulation, and that the "measured lengths" rest only on one or a few single sides of triangles, *actually measured in the common* *acceptation of the term.* Such a side is named the *base*, and from its linear dimensions the linear dimensions of all the other sides of those triangles, by means of which the entire operation is carried out, are *computed.* Let it not be thought that this is an unsatisfactory, or even an unsafe, mode of proceeding. On the contrary, it is a method admitting of a truly wonderful degree of precision; and it does not seldom happen, after a distance of fifty or a hundred miles, and more, having been thus linearly determined, that, on a verification of the computed extent of the last side of the last triangle, the difference is found not to exceed one single foot. Such results are, indeed, triumphs of scientific skill, and well may Von Zach * state in reference to them, that "in his judgment, they prove more in favor of the admirable execution and correctness of a geodetic operation, than all astronomical determinations of longitude and latitude taken together."

But the geometrical *right-line,* by which thus the linear dimensions of the *curved* surface of the Earth are determined, *represents in different latitudes different values,* arising out of the difference of curvature at the respective latitudes. Thus, the same right line, which constitutes the geometrical *base* of a geodetic measurement, and from which the

* Monatl. Correspondenz, vol. xxviii., p. 141.

linear values of all the other sides of triangles, or all the distances are computed, will, towards the poles, where the meridional circular curvature is less, represent also a less extent of meridional curved surface of the Earth, than it will at the equator. Hence it is plain, that these differing measures, for the sake of comparison, have to be reduced to one common meridional—the equatorial—measure. If we term the length of the equatorial meridian degree L, and l that of a meridian degree in any given latitude, the expression of the difference is—

$$(l-L).\ \frac{\pi}{180},$$

and additive. For degrees of parallels, with the base in the parallel, it is—

$$\frac{(l-L).\ \frac{\pi}{180},}{\cos.\ \phi}$$

and subtractive. In either case it reaches the maximum of its value at the poles, while at the equator it becomes = 0. Thus, *f. i.*, at Staronekrasofka, in latitude 45° 20′ 20″·8, the meridional difference for curvature is—

$$l = 57{,}037^{t}{\cdot}9$$
$$L = 56{,}726{\cdot}3$$

$$311^{t}{\cdot}6 = \log.\ 2{\cdot}4936$$
$$\frac{\pi}{180} = \text{,,}\ \bar{2}{\cdot}2419$$

$$5^{t}{\cdot}4 = \log.\ 0{\cdot}7355$$

On addition of the differences between the measured and the computed values of the Russian arcs of our table, we find the sum to be—

$$
\begin{array}{r}
+\ 133^{t}{\cdot}4 \\
-\ 132\ {\cdot}2 \\
\hline
12)\ +\ \ \ 1^{t}{\cdot}2\,;
\end{array}
$$

and, therefore, the mean difference $= + 0^{\cdot t}1$, or a little more than seven inches and a half in a degree. Taking, then, the amplitude of the whole arc, or the astronomical latitudes of Fuglenaes and Staronekrasofka to be correctly determined, the question arisés, whether the differences, which the single arcs represent, are owing to an error of geodetic measurement, of triangulation, or of astronomical determination of the latitude of the intermediate stations. The latter supposition is more probably the true one. "Since it has been found," Von Zach observes,* "how difficult it is to determine geographical latitudes within a few seconds, even by means of thousands of observations, these observations can no longer be regarded as certain tests of the accuracy of geodetic measurements." Indeed, so fully is this admitted, that, *f. i.*, Professor Encke, the present Astronomer Royal of Berlin, applies,† on the ground of geodetic measurements, the following corrections to the observed astronomical latitudes, by which the Cape arc is determined:—

* Monatl. Correspondenz, vol. xxviii. p. 135.

† Berliner Jahrbuch für 1852, pp. 340—1.

	Observed Latitudes.			Corrected Latitudes.			Correction.
	°	′	″	°	′	″	″
Royal Observatory	−33	56	3·0	−33	56	7·59	−4·59
Klyp Fontein	−32	42	0·98	−32	41	57·47	+3·51
Heerelogements Berg......	−31	58	5·63	−31	58	9·87	−4·24
Kamies Berg	−30	21	28·26	−30	21	22·93	+5·33

"The signs of the corrections applied," Professor Encke remarks, "answer to the local condition of the country, as described by Mr. Maclear. As the Royal Observatory immediately faces the Table Mountain towards the south, the result, in consequence of the plumb-line being attracted by it, is that the southern latitude was observed too small. Klyp Fontein having high mountains to the north, the sign of correction changes. Heerelogements Berg, again, has mountains to the south; and Kamies Berg nearer mountains to the north than to the south." It is not, however, to the imaginary character of this explanation or to the value of the single corrections, that I would call attention, but to the circumstance, that Professor Encke does not hesitate to *extend the amplitude* of that small measured arc, by 9″·92, or *nearly ten seconds in arc*, making it 3° 34′ 44″·66 instead of 3° 34′ 34″·74.

We may now revert to the Russian arc. Supposing both its extremities and, consequently, its whole amplitude, as well as its linear extent throughout, to have been correctly determined, and computing the astronomical latitude of the intermediate stations from the geodetic measurement, we obtain the results, comprehended in the following table :—

No. of Arc.	Stations.	Corrected Latitudes.	Resulting Differences with Observation.		
			Latitude.	Amplitude of Arc.	Length of a Degree.
		° ′ ″	″	″	Toises.
	Fuglenaes	70 40 11·3	0·0		
1				+ 1·2	− 0·6
	Stor-Oiwi	68 40 57·2	− 1·2		
2				− 5·4	− 1·0
	Torneå	65 49 48·9	+ 4·5		
3				+ 4·5	− 0·8
	Kilpi-Maeggi	62 38 4·7	− 0·3		
4				− 0·65	− 0·8
	Hochland	60 5 10·4	+ 0·35		
5				+ 0·65	0·0
	Dorpat	58 22 47·3	− 0·3		
6				− 4·1	− 0·8
	Jacobstadt	56 30 8·6	+ 3·8		
7				+ 4·2	− 0·8
	Niemèz	54 39 5·5	− 0·4		
8				− 2·3	− 0·8
	Belin	52 2 44·1	+ 1·9		
9				+ 2·1	− 0·8
	Kremenec	50 5 49·8	− 0·2		
10				− 5·0	0·0
	Suprunkofcy	48 45 7·9	+ 4·8		
11				+ 1·3	0·0
	Wodolui	47 1 28·7	+ 3·5		
12				+ 3·5	0·0
	Staronekrasofka	45 20 2·8	0·0		

SCANDINAVIAN AND NORTH-GERMAN ARCS.

No.	Stations.	Observed Latitudes.	Amplitude of Arc.	Mean Latitude.
		° ′ ″	° ′ ″	° ′ ″
13	Pahtavara Mallörn	67 8 49·7 65 31 30·4	1 37 19·3	66 20 10·0
14	Kippis Torneå	66 48 28·3 65 50 50·0	0 57 38·7	66 19 39·2
15	Memel Trunz	55 43 40·5 54 13 11·5	1 30 29·0	54 58 26·0
16	Lysabbel Lauenburg	54 54 10·3 53 22 17·0	1 31 53·3	54 8 13·7
17	Altona Göttingen	53 32 45·3 51 31 47·9	2 0 57·4	52 32 16·6

These results, which, though in more than one sense of a very remarkable character, can here no further be discussed, prove, under any circumstances, the extraordinary degree of accuracy and consistency, with which this extensive geodetic operation has been carried out. Well may the Russian nation look with pride upon such an enterprise, and upon the men, who have conducted it to so successful a termination.

Our next table contains the Swedish, Danish, and North-German arcs, which, presenting neither continuity nor any great extent, and being, therefore, of less theoretical importance, may be grouped together.

SCANDINAVIAN AND NORTH-GERMAN ARCS.

No.	Radius Vector.	Measured Length of Arc.	Length of a Degree. Measured.	Length of a Degree. Computed.	Differences.
	Toises.	Toises.	Toises.	Toises.	Toises.
13	3,278,906	92,755·9	57,197·5 + 8·2	57,227·7	− 22·0
14	3,278,897	55,023·5 − 60·4	57,208·5 + 8·2	57,227·5	− 10·8
15	3,273,161	86,175·9	57,143·7 + 7·0	57,127·4	+ 23·3
16	3,272,687	87,434·5	57,091·8 + 6·9	57,119·2	− 20·5
17	3,271,775	115,161·2	57,125·2 + 6·6	57,103·3	+ 28·5

On adding the differences between the observed and the computed lengths of degrees, we find the sum to be—

$$\begin{array}{r} -\ 53^{t.}\ 3 \\ +\ 51\cdot 8 \\ \hline 5)\ -\ 1^{t.}\ 5, \end{array}$$

and, hence, the mean difference = − $0.^{t}30$; an agreement, however, which, as so *close* an agreement, has to be considered as a matter of chance, rather than anything else.

None of the above arcs require any special remark, except the arc No. 14, known as Maupertuis' Swedish arc, because measured by him, at the command of the French Government, in 1736—1737, in connection with the measurement of the Peruvian arc by Bouguer and La Condamine. The results have been frequently discussed and examined. Mr. Galloway observes*:—"Maupertuis' arc was never reckoned of much value, as it was evident from the first, that there was an error in the determination of the latitudes." You yourself, on the contrary, remark:†—"In order to make this measure and that of Svanberg agree in all points, it is necessary to suppose an error of 12″ or more in the French observations of latitude at Kittis. From the excellence of the instrument, the reputation of its maker,

* "Figure of the Earth," p. 563. † Ibid. p. 206.

the care and fidelity of the observers, as shown in the points that have been examined, and the circumstance of their having repeated the observations under the fear that some error had crept into the first set, we have no hesitation in expressing our opinion that this is *impossible.* And we are glad to find that Mr. Rosenberger, after a careful examination of all the observations (see the *Astronomische Nachrichten,* Nos. 121, 122), has come to the same conclusion. From the difference of the altitudes of α and δ Draconis, compared with their known difference of declination, he has shown that the line of collimation was in the same state before the journey from and after the return to Kittis. And from observations made in France with the same instrument (see the 'Degré du Méridien entre Paris et Amiens'), it appears that after repeated voyages the line of collimation was not sensibly changed. The length of the arc which Mr. Rosenberger is inclined to adopt, is 55,020·16 toises, and the difference of latitude 57′ 30″·44; whence 1°=57,405^{t}·02. The mean latitude is 66° 19′ 37″."

These values, differing from the results, as given by Maupertuis, by + 1″·77 in astronomical amplitude and—32·9 toises in linear extent, are adopted also by you, and accompanied by the remark: "Mountainous country. A very little doubt about the amplitude." The "error in measure," you thus find, is for the arc in question + 1217 English feet = 190·3 toises, or nearly 200 toises in a degree; that is to say, as computed by *empirical* formulas, and at variance with the astronomical *theory*, according to which that difference amounts to 455 toises. The difference, I find, being only − 10·ᵗ8 in a degree, bears a full, though late testimony to the skill and accuracy, as well as to the care and fidelity, of the distinguished French academician. For, there is no error either of geodetic or of astronomical measurement. But there is an essential error of reduction, as regards the latter; and an error, too, by no means of an isolated character in the history of modern geodesy, although it has escaped your critical attention, and that of astronomers generally.

The mode of determining the difference of latitude between Mount Kittis and Torneå, by means of a sector (made by Graham), is thus described by Maupertuis:* — "Observation of the star δ Draconis on the Kittis, Oct. 4, 1736.

"Previously to the observation of the star's meridian-passage, the plumb-line having been placed,"—I translate literally,— "on the point of the limb, marked 2° 37′ 30″,

* "La Figure de la Terre" (Amst.), pp. 112—118.

	Rev.	Parts.
of the upper division, which has invariably been used, the micrometer indicated ...	24	10·7

44 of which parts are equal to one revolution.

"During the observation, that is to say, at the star's meridian-passage, the micrometer marked	22	30·9
"After the observation, the same point, 2° 37′ 30″, having been placed under the plumb-line, the micrometer indicated ...	24	12·5
"Taking the mean of the micrometer readings before and after the star's passage, we have	24	11·6
and subtracting from this	22	30·9
we find, in divisions of the micrometer, the arc, comprised between the point of the limb marked 2° 37′ 30″, and that, to which the plumb-line corresponded at the star's passage, to be	1	24·7."

These observations were repeated on Mount Kittis on the 6th, 8th, and 10th of October. The sector was then transferred to Torneå, where the same star δ Draconis was observed, in the same manner, from the 1st to the 5th of October, with "the plumb-line on that point of the limb, marking 1° 37′ 30″." The mean result of these observations at the two extremities of the measured arc having been adopted, the astronomical amplitude of that arc was thereupon computed as follows.

" We have, consequently," Maupertuis continues, " for the arc of the limb, marked by the plumb-line at the star's passage over the meridian of Kittis	° ′ ″ Rev. Parts. 2 37 30 − 1 25·8
and for the arc of the limb marked by the plumb-line at the star's passage over the meridian of Torneå	1 37 30 + 1 40·6
" The difference between these two arcs gives the difference of the star's distances from the zenith of Kittis and Torneå =	1 0 0 − 3 22·4
and reducing the parts of the micrometer to minutes and seconds, in the proportion of $15' : 20^{r.}\ 23{\cdot}5$, we find $3^{r.}\ 22{\cdot}4$ =	$2'\ 33''{\cdot}8$
which being deducted from ...	$1^{\circ}\ 0\ \ 0{\cdot}0$
gives the amplitude of the measured arc ... ... =	$0^{\circ}\ 57'\ 26''{\cdot}2$."

In a similar manner, the verification of this result was effected by observations of the star α Draconis, first at Torneå, and then on the Kittis. The corrected results were found to be $57'\ 26''{\cdot}93$ and $57'\ 30''{\cdot}42$; and, refraction, as " certainly producing here no sensible effect," being neglected, their mean of 57′ 28″·67 was finally adopted.

Now, in order to place the error of principle, involved in this mode of observing, or rather in reducing the observations, in a more striking light, let us imagine an arc of 90° to have been similarly determined at two stations, the one at the equator, the other at the pole, by observations of the same star in or near the zenith of either station. It is manifest, that the same method of reduction, followed by Maupertuis, would make the observed arc of 90° only 89° 45′; the error involved in it, in connection with the mode of observation, being exactly 15′, or, generally, as 1 : 360. Hence, the geodetic arc, astronomically determined by Maupertuis, comprises, if we accept his own direct result of 57′ 28″·7 as correct, $\frac{57' \; 28''\cdot 7}{360} = 9''\cdot 3$ more than he computed it to be; and further adding 0″·7 for refraction, we find the true amplitude of the measured arc to be 57′ 38″·7.

But the result of the geodetic measurement, also, requires a correction, which has been neglected. The linear extent of the base, namely, was measured, on the frozen river, by rods of fir-wood, which had been compared with an iron toise at the temperature of + 15° Reaumur. It is true, Maupertuis himself states, that his experiments indicated hardly sensible variations in the length of these rods from a difference of temperature; but it is a matter of observation that, however insensibly, they do contract by the cold;* and, even for Lapland, the cold was at the time unusually intense. "Le froid fut si grand dans le mois de Janvier" (1736), Maupertuis relates,† "que nos thermomètres de mercure, de la construction de M. de Reaumur, ces thermomètres qu'on fut surpris de voir descendre à 14 degrés au-dessous de la congélation à Paris dans les plus grands froids du grand hiver de 1709, descendirent alors à 37 degrés: ceux d'esprit de vin gelèrent." The measurement of the base had been completed only a few days previously. But, the linear extent of fir-wood at 0° Celsius being taken as unity, it has been found to extend and contract between the temperature of the freezing and the boiling points of water, by ·0004959, according to Kater; by ·0003520, according to Von Struve; by ·0003000, according to Parry. We cannot therefore, I think, be far wrong in applying the value of ·0003 to Maupertuis' arc, which would

* Similar deal-rods were made use of, at first, by General Roy, in the English survey; but they were found liable to variations of length so considerable, that General Roy abandoned them after the base on Hounslow Heath had been completed.

† "Figure de la Terre," p. 66.

reduce the measured distance of 55,023t·5, as computed by him, to 54,963·1 toises.

The English arc, extending from latitude 49° 53′ 33″·9 to latitude 60° 49′ 38″·6, comprises a distance in arc of 10° 56′ 4″·7 only; but, as regards the perfection of the instruments used in the measurement, the time, care, and scientific skill expended upon that great national enterprise, the Ordnance Survey of the United Kingdom, which has furnished its elements, it stands, probably, unrivalled.

In the following table, the astronomically observed latitudes, as given at p. 686 of the "Ordnance Survey," are combined with the non-corrected distances of parallels at p. 732. I have divided the entire arc into ten sections, each comprising a degree more or less, and the number of stations including four observatories. The given distances for Calton Hill, both the non-corrected and the corrected, are affected with an error of 10,000 feet; the correct numbers, instead of 220,3487·7 and 220,3477·2 feet, being 221,3487·7 and 221,3477·2 feet. Hence, the distance between Durham and Calton Hill is 433,472·7, instead of 423,472·7 feet; and the distance between Calton Hill and Great Stirling, 551,211·4, instead of 561,211·4 feet.*

* At p. 742 of the "Ordnance Survey," with reference to the

ENGLISH ARCS.

No.	Stations.	Observed Latitudes.	Amplitude of Arc.	Mean Latitude.
		° ′ ″	° ′ ″	° ′ ″
	Saxavord	60 49 38·58		
18			1 42 23·39	59 58 26·7
	North Rona......	59 7 15·19		
19			0 45 54·35	58 44 18·0
	Monach	58 21 20·84		
20			0 53 31·72	57 54 35·0
	Great Stirling ...	57 27 49·12		
21			1 30 25·98	56 42 36·1
	Calton Hill	55 57 23·14		
22			1 11 16·94	55 21 44·7
	Durham	54 46 6·20		
23			1 18 35·80	54 6 48·3
	Clifton............	53 27 30·40		
24			1 14 38·77	52 50 11·0
	Cambridge	52 12 51·63		
25			0 44 13·33	51 50 45·0
	Greenwich	51 28 38·30		
26			0 51 31·22	51 2 52·5
	Dunnose	50 37 7·08		
27			0 43 33·15	50 15 20·5
	St. Agnes.........	49 53 33·93		

final table of the English arc, it is remarked :—" The number of points having astronomical latitudes is so great that we may dispense with some of them without fear of lessening the accuracy of the final results. We shall, therefore, omit Cowhythe, Calton Hill (Royal Observatory, Edinburgh), and all the Irish points." Strange enough, the distances of 2844909·0 and 2844897·6 feet, given at page 732, for Cowhythe, are likewise erroneous to the extent of 1,000 (or nearly 1,000) feet; but, as this station is not included in my table, I have not verified the latter error by a more accurate calculation.

ENGLISH ARCS.

No.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
			Measured.	Computed.	
	Toises.	English Feet.	Toises.	Toises.	Toises.
18	3,275,855	623,801·3	57,164·7 + 7·8	57,174·4	− 1·9
19	3,275,210	279,559·5	57,140·7 + 7·6	57,163·2	− 14·9
20	3,274,771	326,135·6	57,167·6 + 7·5	57,155·5	+ 19·6
21	3,274,120	551,211·4	57,191·2 + 7·3	57,144·2	+ 54·3
22	3,273,376	433,472·7	57,058·2 + 7·1	57,131·1	− 65·8
23	3,272,674	478,573·8	57,132·5 + 6·9	57,118·9	+ 20·5
24	3,271,946	453,922·8	57,057·5 + 6·6	57,106·2	− 42·1
25	3,271,375	268,941·8	57,063·2 + 6·5	57,096·3	− 26·6
26	3,270,911	313,716·9	57,134·4 + 6·3	57,088·2	+ 52·5
27	3,270,446	264,859·7	57,061·2 + 6·2	57,080·1	− 12·7

Upon adding the differences in this table, we find the sum to be

$$
\begin{array}{r}
-\ 164^{t \cdot}\ 0 \\
+\ 146 \cdot 9 \\
\hline
\end{array}
$$

10) $-$ 17$^{t\cdot}$ 1; and, consequently the mean difference = $-$ 1$^{t\cdot}$71 in a degree, which, as compared with the mean difference of the

Russian arc, might be a more satisfactory one. That it is not, however, owing to the geodetical measurement, is readily proved. The mean latitude of the entire arc, as geodetically determined, is 55° 21′ 36″·4. For this latitude, according to our table, p. 119, the difference between the true dimensions of a meridian degree, and the dimensions deduced from a correct measurement and the astronomical amplitude of arc, as calculated from that measurement with the erroneous elements of the present theory, should be — 17ᵗ·2 in a degree. The geodetic measurement, therefore, will be in error to the extent of its deviation from this quantity: assuming the polar elongation of the Earth = $\frac{1}{95}$, and the length of an equatorial degree = 56,726ᵗ·3, have been correctly determined. Now, the lati- of the two extremities of the arc, geodetically uted, are,—

		°	′	″
Saxavord.........	=	60	49	41·992
St. Agnes	=	49	53	30·776

and, hence, amplitude of the arc =10° 56′ 11″·216; while the r distance between the two parallels is 3,994,195·5 ish feet.* From this, we find the measured

* "Ordnance Survey," p. 732. The "Correction" there applied

length of a degree for the latitude of 55° 21′ 36″·4 to be

57,113^{t}·7, while its true or computed length is 57,131^{t}·2. Thus the difference of

—17^{t}·5, which exceeds the normal quantity by only 0^{t}·3, shows that the geodetic result of the entire operation very closely accords with a true theory, and, therefore, may be regarded as virtually correct. Deducting from that difference the value of 7^{t}·1 for difference of curvature in latitude 55° 21′ 36″, there

to this quantity is — 12·8 feet. The following are the "corrected" or "final results" adopted :—

DISTANCES OF PARALLELS.

Stations.	Final Results.	Stations.	Final Results.
Saint Agnes......	0·0	South Berule ...	1554040·0
Goonhilly.........	56206·0	Tawnaghmore ...	1607014·3
Hensbarrow......	179204·1	Burleigh Moor...	1708025·3
High Port Cliff	256678·4	Durham	1780002·4
Week Down ...	257399·2	Lough Foyle Base	1880308·1
Boniface Down .	259360·8	Calton Hill	2203477·2
Dunnose	264846·9	Ben Lomond ...	2299441·9
Black Down......	289839·8	Kellie Law	2320585·6
Southampton ...	372654·2	Ben Heynish ...	2396289·0
Greenwich	578564·2	Great Stirling ...	2764686·7
Hungry Hill ...	654959·3	Cowhythe	2844897·6
Feaghmaan	741026·0	Monach	3090834·5
Precelly	749557·8	Ben Hutig	3162038·7
Cambridge	847505·9	North Rona......	3370391·8
Arbury Hill......	851203·2	Balta	3966276·5
Forth	884632·0	Gerth of Scaw...	3990103·5
Delamere.........	1215251·2	Saxavord	3994182·7
Clifton Beacon...	1301428·5		

remains a difference of 10^{t}·4 in a degree, by which the geodetically computed amplitude of the arc has been determined in excess of its true amplitude. Reducing, then, the former amplitude of 10° 56′ 11″·216 accordingly, we find its correct value, as geodetically computed, to be

10° 56′ 4″·06. The observed amplitude is 10° 56′ 4″·65; and, consequently, there is a difference of only 0″·59 between the general geodetical and astronomical results of the entire operation, nothing than which could be more satisfactory.

Adopting the astronomically observed latitudes of St. Agnes and Saxavord as correct, we have to deduct from the logarithms of the geodetically computed amplitudes of the single arcs, expressed in seconds, the logarithmic difference between the astronomical and the erroneously computed geodetical amplitudes of the entire arc,—

10° 56′ 11″·216	=	39,371″·22	=	log. 4·5951788
10 56′ 4 · 65	=	39,364 ·65	=	„ 4·5951063
			=	log. 0·0000725

in order to reduce the latter to their true geodetically computed values. Calculating with these values and the non-corrected distances of parallels, the "measured lengths of degrees," as compared with their theoretical lengths computed on the new theory, we obtain the results embodied in the following table.

No.	Stations.	Geodetically computed. Amplitude of Arc.	Geodetically computed. Latitudes.	Geodetically computed. Mean Latitude.	Radius Vector.	Measured Length of Arc.	Length of a Degree. Measured.	Length of a Degree. Computed.	Differences.
		° ′ ″	° ′ ″	° ′ ″	Toises.	English Feet.	Toises.	Toises.	Toises.
	Saxavord		60 49 38·58						
18		1 42 23·29		59 58 26·9	3,275,855	623,801·3	57,165·5 + 7·8	57,174·4	− 1·1
	North Rona		59 7 15·29						
19		0 45 53·67		58 44 18·5	3,275,210	279,559·5	57,154·7 + 7·6	57,163·2	− 0·9
	Monach........		58 21 21·62						
20		0 53 32·86		57 54 35·2	3,274,771	326,135·6	57,147·3 + 7·5	57,155·5	− 0·7
	Great Stirling ..		57 27 48·76						
21		1 30 31·19		56 42 33·2	3,274,120	551,211·4	57,136·3 + 7·3	57,144·2	− 0·6
	Calton Hill		55 57 17·57						
22		1 11 12·03		55 21 41·6	3,273,376	433,472·7	57,123·8 + 7·1	57,131·1	− 0·2
	Durham........		54 46 5·54						
23		1 18 37·49		54 6 46·8	3,272,674	478,573·8	57,112·0 + 6·9	57,118·9	± 0·0
	Clifton		53 27 28·05						
24		1 14 35·45		52 50 10·3	3,271,946	453,922·8	57,099·8 + 6·6	57,106·2	+ 0·2
	Cambridge		52 12 52·60						
25		0 44 12·08		51 50 46·6	3,271,375	268,941·8	57,090·2 + 6·5	57,096·3	+ 0·4
	Greenwich		51 28 40·52						
26		0 51 34·04		51 2 53·5	3,270,911	313,716·9	57,082·4 + 6·3	57,088·2	+ 0·5
	Dunnose		50 37 6·48						
27		0 43 32·54		50 15 20·2	3,270,446	264,859·7	57,074·6 + 6·2	57,080·1	+ 0·7
	St. Agnes......		49 53 33·94						

The differences, which this table exhibits, speak for themselves, and show the great accuracy of the measured lengths of arcs; the impropriety of the "Corrections" applied to them in the Ordnance Survey; and the consistency, though not the absolute truth, of the computed latitudes. On comparing the latter with the observed latitudes, there appear the following differences:—

		"			"
Saxavord	±	0·00	Clifton	—	2·35
North Rona......	+	0·10	Cambridge	+	0·97
Monach	+	0·78	Greenwich	+	2·22
Great Stirling ...	—	0·36	Dunnose	—	0·60
Calton Hill	—	5·57	St. Agnes.........	±	0·00
Durham	—	0·66			

A considerable portion of the Account of the Ordnance Survey, and much labour, have been devoted to the attempt of explaining these discrepancies by the deflection of the plumb-line in consequence of local attraction—a deviation within a deviation. This, however, is a question into which I need not here enter; although I may be permitted to remark that the explanation offered, is readily proved *not* to be the true one.

If we compute from the slightly "corrected" latitudes, finally adopted in the Ordnance Survey, p. 743, and the non-corrected distances of parallels, the measured lengths of a degree, and compare them with the theoretical lengths, the differences are found to be as follows :—

Arc No. 18.	— $1^{t}\cdot 9$		Arc No. 23.	+ $9^{t}\cdot 6$
19.	— 5 ·3		24.	— 30 ·6
20.	+ 11 ·2		25.	— 26 ·6
21.	+ 54 ·3		26.	+ 42 ·5
22.	— 65 ·8		27.	— 0 ·9

the mean difference being — $1^{t}\cdot 35$ in a degree. The "Corrections," applied to the distances of parallels, affect sensibly only the arcs Nos. 18, 19, 20, and 27, for which the differences become respectively, $-2^{t}\cdot 9$, $-5^{t}\cdot 7$, $+13^{t}\cdot 4$, and $-3^{t}\cdot 6$. Hence, the mean difference is increased to — $1^{t}\cdot 66$; and the sole tendency of these "Corrections" to distances, is to compensate for the "Corrections" to latitudes, just mentioned, so as to make the mean result, obtained from the "corrected" elements, as nearly as may be, equal to the mean results derived from the uncorrected elements.

The modern French arc from Formentera to Dunkirk, and thence extended to Greenwich, embracing an arc of nearly 13° between 38° 39′ 39″·03 and 51° 28′ 38″·20 of north latitude, holds one of the foremost places in meridional geodesy. It still, however, continues in a very unsatisfactory state, on account of the doubtful determination of the latitude of some of the principal stations, including those of both its proper extremities. This will appear at once from the following table :—

FRENCH ARCS.				
No.	Stations.	Observed Latitudes.	Amplitude of Arc.	Mean Latitude.
		° ′ ″	° ′ ″	° ′ ″
	Greenwich	51 28 40·0		
28			0 26 31·5	51 15 24·3
	Dunkirk	51 2 8·5		
29			2 11 19·1	49 56 28·9
	Paris (the Pantheon)	48 50 49·4		
30			2 40 6·9	47 30 45·9
	Evaux	46 10 42·5		
31			2 57 48·2	44 41 48·4
	Carcassonne......	43 12 54·3		
32			1 51 7·7	42 17 20·5
	Montjouy.........	41 21 46·6		
33			2 41 53·4	40 0 49·9
	Formentera	38 39 53·2		
	Greenwich	51 28 38·20		
28-33			12 48 39·03	45 4 18·6
	Formentera	38 39 39·03		

I have, with Biot and Arago, extended the arc to Greenwich, because the reason, on the strength of which you protest against such an extension,* is utterly inapplicable; and, the latitude of Greenwich being well established, the uncertainty, attaching to the latitude of one at least of the proper extremities of the arc, is thus removed. You yourself remark :†—

* "Figure of the Earth," p. 209. † Ibid. p. 210.

FRENCH ARCS.

No.	Radius Vector.	Measured Length of Arc.	Length of a Degree. Measured.	Length of a Degree. Computed.	Differences.
	Toises.	Toises.	Toises.	Toises.	Toises.
28	3,271,033	25,241·9	57,097·6 + 6·4	57,090·3	+ 13·7
29	3,270,262	124,944·8	57,087·7 + 6·3	57,088·1	+ 5·9
30	3,268,826	152,293·1	57,069·3 + 5·7	57,051·8	+ 23·2
31	3,267,147	168,846·7	56,977·4 + 5·4	57,022·5	− 39·9
32	3,265,711	105,499·0	56,960·5 + 4·7	56,997·5	− 32·3
33	3,264,365	153,675·3	56,955·4 + 4·3	56,974·0	− 14·3
28-33	3,267,371	730,500·8	57,022·1 + 5·2	57,026·4	+ 0·9

"It is much to be wished that the latitudes of Dunkirk and Formentera were determined with greater certainty. The latter rests on an enormous number of observations, but entirely on stars north of the zenith, and with a circle whose level was attached to the axis." This remark includes so far an essential error on your part, as the observations in question were by no means confined to stars north of the zenith. On the contrary, of the entire number of

1,060 observations, no less than 510 refer to stars south of the zenith.* But they were all made between the 7th of June and the 1st of July (1825). The mean latitude of Formentera, deduced from them, is 38° 39′ 53″·172. You adopt, with Delambre, 38° 39′ 56″·1. For reasons, however, which I cannot enter into on the present occasion, the two series of 50 observations of β Ursæ Minoris, and of 20 observations of γ Ursæ Minoris, deserve here the preference. The mean result of the former was 38° 39′ 58″·302, that of the latter 38° 40′ 0″·039; the mean of both being 38° 39′ 59″·17.

Adopting, then, this latitude for Formentera and the latitude of 51° 28′ 38″·20 for Greenwich, we find the amplitude of the entire arc to be 12° 48′ 39″·03, which gives the length of a degree for the mean latitude of the arc of 45° 4′ 18·″6 = 57,027ᵗ·3, *differing only by the length of* + 0ᵗ·9 *from the length computed on the new theory*—an agreement which again offers a brilliant testimony to the accuracy of the *geodetic* operations, and which, from the exemplary care and the consummate skill of the eminent French astronomers, Delambre, Méchain, Arago, and Biot, who conducted the undertaking, was to be expected.

* Biot, "Traité élém. d'Astronomie Physique," tom. iii., p. 521, table D.

Calculating now the latitudes of the intermediate stations from the geodetic measurements, we obtain the following results:—

No. of Arc.	Stations.	Corrected Latitudes.	Resulting Differences with Observation.		
			Latitude.	Amplitude of arc.	Length of a Degree.
		° ′ ″	″	″	Toises.
28	Greenwich	51 28 38·2	− 1·8	+ 0·4	− 0·6
29	Dunkirk	51 2 6·3	− 2·2	+ 0·8	+ 0·3
30	Paris...............	48 50 46·4	− 3·0	+ 4·2	− 0·2
31	Evaux	46 10 35·3	− 7·2	− 7·4	0·0
32	Carcassonne	43 12 54·5	+ 0·2	− 3·2	− 0·7
33	Montjouy	41 21 50·0	+ 3·4	− 2·5	+ 0·5
	Formentera	38 39 59·1	+ 5·9		

The Indian arcs, measured by Colonel Lambton and Captain Everest, yield in importance and admirable execution to none, in extent only to the Russian arcs. The following table embodies the results, as compared with those computed on the new theory. On addition of the differences, the sum is found to be,—

$$\begin{array}{r} -\ 77^{t}{\cdot}0 \\ +\ 76\ {\cdot}9 \\ \hline 7)\ -0^{t}{\cdot}1 \end{array}$$

; and, therefore, the mean difference = − $0^{t}{\cdot}014$—which leaves nothing to desire, amounting, as it does, to *one inch* in a degree only.

INDIAN ARCS.

No.	Stations.	Observed Latitudes.	Amplitude of Arc.	Mean Latitude.
		° ′ ″	° ′ ″	° ′ ″
	Kulliampoor	24 7 11·8		
34			3 1 19·9	22 36 31·9
	Takal Khera	21 5 51·9		
35			3 2 35·8	19 34 34·0
	Daumeragidda...	18 3 16·1		
36			2 57 21·8	16 34 35·2
	Namthabad	15 5 54·3		
37			2 6 1·4	14 2 53·6
	Dodagoontah	12 59 52·9		
38			2 0 9·9	11 59 48·0
	Putchapollium ...	10 59 43·0		
39			2 50 10·5	9 34 37·7
	Punnae	8 9 32·5		
	Paudree	13 19 49·0		
40			1 34 56·4	12 32 20·8
	Trivandeporum ...	11 44 52·6		

In the following table the Peruvian and Cape arcs are grouped together.

The measurement of the arc at the Cape of Good Hope by Mr. Maclear does not appear of a nature to inspire great confidence in its accuracy. Not only are the differences unusually great, but the mean difference, also, is comparatively considerable. Still, it is very probable that in this case, too, the geodetic part of the operation is sufficiently satisfactory, and that the discrepancies arise chiefly from errors in

INDIAN ARCS.

No.	Radius Vector.	Measured Length of Arc.	Length of a Degree. Measured.	Length of a Degree. Computed.	Differences.
	Toises.	Toises.	Toises.	Toises.	Toises.
34	3,255,256	171,593·2	56,779·9 + 1·6	56,814·9	— 34·3
35	3,254,036	172,876·2	56,805·9 + 1·2	56,793·6	+ 13·5
36	3,252,975	167,858·0	56,784·6 + 0·9	56,775·2	+ 10·3
37	3,252,204	119,130·8	56,718·4 + 0·6	56,761·7	— 42·7
38	3,251,664	113,747·3	56,795·6 + 0·5	56,753·6	+ 42·5
39	3,251,130	160,940·2	56,744·0 + 0·3	56,742·9	+ 1·4
40	3,251,798	89,818·8	56,763·5 + 0·5	56,754·6	+ 9·2

astronomical latitude. Thus, when we deduce the latitude of Klyp-Fontein from Mr. Maclear's data, we find it to be—

32° 41′ 51″·3 from Lacaille's triangles and modern base;

41 52 ·7 from modern remeasurement, geodetic and astronomical;

42 1 ·0 from Maclear's arc; the difference even between Mr. Maclear's own determinations thus amounting to 8″·3.

CENTRAL-AMERICAN AND

No.	Stations.	Observed Latitudes.	Amplitude of Arc.	Mean Latitude.
		° ′ ″	° ′ ″	° ′ ″
	Cotchesqui.........	+0 2 32·7		
41			3 7 3·5	1 31 0·4
	Tarqui	−3 4 30·8		
	Kamies Berg	30 21 28·3		
42			1 36 37·4	31 9 47·0
	Heerelogements Berg	31 58 5·7		
43			0 43 55·3	32 20 3·3
	Klyp-Fontein ...	32 42 1·0		
44			1 14 2·0	33 49 2·0
	Cape Observatory	33 56 3·0		

I may here be permitted to point out an inadvertent error, into which Sir John Herschel has fallen. In his "Outlines of Astronomy," the middle latitude of Maclear's arc is stated to be 35° 43′ 20″,—a latitude, which the distinguished astronomer of Collingwood would seem to have arrived at, by *adding* half the amplitude of the arc, =1° 47′ 17″, to the latitude of the Cape Observatory, =33° 56′ 3″.

The meridian arcs, the measured lengths of which have thus been compared with the corresponding lengths, computed on the new theory of the Earth's figure, comprise an extent of 80° 57′ 23″·8, distributed on various meridians between 70° 40′ 11″·3 of north,

SOUTH-AFRICAN ARCS.					
No.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
			Measured.	Computed.	
	Toises.	Toises.	Toises.	Toises.	Toises.
41	3,250,202	176,852·2	56,727·7	56,726·3	+ 1·4
42	3,259,375	91,747·2	56,972·1 + 2·8	56,886·8	+ 88·1
43	3,260,001	41,522·5	56,722·6 + 3·0	56,897·8	−172·2
44	3,260,813	70,334·0	57,001·8 + 3·2	56,911·9	+ 93·1

and 33° 56′ 3″·0 of south, latitude. The mean differences, found between theory and measurement are :—

		In a degree.
I.	12 Russian arcs	+ 0^{t}·100
II.	5 Scandinavian and North-German arcs	− 0 ·300
III.	10 English arcs	− 1 ·710
IV.	6 French arcs	+ 0 ·900
V.	7 Indian arcs	− 0 ·014
VI.	4 Central-American and South-African arcs	+ 2 ·600

44 arcs ; the mean difference of all of which is 0^{t}·11, or *eight inches* in a degree.

It may possibly be asserted, that the present theory represents the measured distances with equal, or almost equal, accuracy. No assertion could be more erroneous. There even does not, in modern astronomy, exist *a theory* of the Earth's figure, properly speaking. Astronomers, certainly, have succeeded, and naturally so, in computing, by the aid of empirical formulas and tentative co-efficients, the actual meridional dimensions of the Earth with tolerable exactness; but, as I have already shown, such an agreement is obtained only at the sacrifice of every geometrical rule, and in diametrical opposition to their own principles. For the sake of illustration, let us, on the ground of the corrected latitudes, compare the respective differences relating to the Russian arcs. They are exhibited in the following table, from which it will be seen, that the lengths of degrees, even as computed by means of empirical formulas, differ from the corresponding measured lengths to the extent of

— 13·7 toises in latitude 45° 20′ 2″·8

gradually increasing to

— 20·6 toises in latitude 70° 40′ 11″·3;

while the *theoretical* differences, involved in the present astronomical doctrine concerning the Earth's figure, and which a consistent geometrical calculus would have brought to light, are

— 24·2 toises in latitude 45° 20′ 2″·8,

increasing to no less a value than

— 309·6 toises in latitude 70° 40′ 11″·3.

No. of Arc.	Measured Length of a Degree.	Computed Lengths.			Differences.		
		New Theory.	Modern Astronomy.		New Theory.	Modern Astronomy.	
			By Empirical Formulas. I.	By Theory. II.		By Empirical Formulas. I.	By Theory. II.
1	57,251t·1 —	57,251t·7	57,230t·5	56,941t·5	— 0t·6	— 20t·6	— 309t·6
2	57,233 ·5	57,234 ·5	57,213 ·8	56,946 ·0	— 1 ·0	— 19 ·7	— 287 ·5
3	57,210 ·2	57,211 ·0	57,191 ·0	56,954 ·5	— 0 ·8	— 19 ·2	— 255 ·7
4	57,186 ·0	57,186 ·8	57,167 ·5	56,961 ·0	— 0 ·8	— 18 ·5	— 225 ·0
5	57,167 ·7	57,167 ·7	57,149 ·3	56,968 ·5	0 ·0	— 18 ·4	— 199 ·2
6	57,150 ·3	57,151 ·1	57,133 ·0	56,971 ·0	— 0 ·8	— 17 ·3	— 179 ·3
7	57,132 ·5	57,133 ·3	57,115 ·9	56,979 ·5	— 0 ·8	— 16 ·8	— 153 ·0
8	57,110 ·6	57,111 ·4	57,094 ·7	56,986 ·5	— 0 ·8	— 15 ·9	— 124 ·1
9	57,087 ·6	57,088 ·4	57,072 ·5	56,992 ·0	— 0 ·8	— 15 ·1	— 95 ·6
10	57,071 ·5	57,071 ·5	57,056 ·5	56,998 ·0	0 ·0	— 15 ·0	— 73 ·0
11	57,055 ·7	57,055 ·7	57,041 ·3	57,003 ·5	0 ·0	— 14 ·4	— 52 ·2
12	57,037 ·9	57,037 ·9	57,024 ·2	57,009 ·0	0 ·0	— 13 ·7	— 24 ·2

XVI.

Among the few arcs of *longitude*, which have thus far been measured as such, the so-called Great Arc of the Mean Parallel has the first claim to our attention. It extends from Marennes to Fiume, comprising an arc of 15° 32′ 26″·7 as reduced to the mean longitude of 45° 43′ 12″, not of "precisely 44° 16′ 48″"—the north polar distance of the parallel,—as inadvertently stated by Arago in his "Popular Astronomy," and by Admiral Smyth and Professor Grant repeated in the English translation.* Instead of computing, however, the arc as a whole, and entering into a discussion connected with the reduction, it will be far more satisfactory and conclusive, to calculate those sides of triangles or sections of the arc, which nearly form true arcs of longitude of small extent, and represent in a double line the entire distance measured. The details I take from the work of Colonel Brousseaud, "*Mesure d'un Arc du Parallèle moyen entre le Pole et l'Équateur*" (Limoges, 39, 4to.). Together with the results, they will be nbodied in the following table, to which, for a more omplete understanding, I will prefix the type of

* It is difficult to understand, that those distinguished and accomplished astronomers should have lent to this translation, abounding in the grossest errors, astronomical, linguistical, and grammatical, anything but their name.

calculation, taking the side Granier-Belle Achat (No. 42 of our table) of the triangle Granier-Trélot-Belle Achat by way of illustration. It is this:—

Radius vector to lat. 45° 30′ 9″·9
by table, page 144... = 3,267,629$^{t.}$ = log. 6·5142327
$\frac{\pi}{180}$ = „ $\bar{2}$·2418774

Length of meridian degree ... ... = log. 4·7561101
Cos. 45° 30′ 9″·9 ... ... = „ 9·8456406

Length of a degree of the parallel ... = log. 4·6017507

Computed length of a degree = 39971$^{t.}$·6

Measured length of arc = 38489$^{m.}$·7 ... = log. 4·5853444
1 meter = 0·513074$^{t.}$ ... ... = „ $\bar{1}$·7101800

GB, reduced to toises = log. 4·2955244

GB^2 = log. 8·5910488 = 3899859.. $^{t.}$
Meridian degree ... = „ 4·7561101
Difference of latitude
276″·3 = „ 2·4413809

log. 7·1974910
3600″ „ 3·5563025

d = log. 3·6411885

d^2 = log. 7·2823770 = 0191592..

3708267.. $^{t.}$

3708267.. $^{t.}$ = log. 8·5691709

GB, reduced to mean latitude = $\surd$ = log. 4·2845855
Amplitude of arc = 0°·4803516 = „ $\bar{1}$·6815592

1° = 40089$^{t.}$·1 = log. 4·6030263
Correction ... ... 117 ·0

Measured length of a degree = 39972$^{t.}$·1

GREAT ARC OF THE

No.	Stations.	Latitudes.	Mean Latitude.	Difference in Latitude.	Longitudes.
			° ′ ″	″	
1	Peyrelade ...	51^{g}·1263,93	46 0 39·3	20·5	+ 1^{g}·5006,23
	Sauvagnac ...	51 ·1200,81			+ 0 ·9924,13
2	Negret	51 ·0046,50	45 57 32·3	394·4	+ 2 ·0469,10
	Peyrelade ...	51 ·1263,91			+ 1 ·5006,23
3	Le Monteillier	51 ·0468,82	45 54 43·2	217·4	— 3 ·0419,14
	Le Colombier	50 ·9797,79			— 3 ·8013,07
4	Limonest ...	50 ·9429,71	45 53 43·6	336·8	— 2 ·7139,20
	Le Monteillier	51 ·0468,82			— 3 ·0419,14
5	Montoncelle .	51 ·0337,15	45 53 25·9	286·6	— 1 ·5083,91
	Bossivre	50 ·9452,69			— 2 ·2715,57
6	San Salvatore	50 ·9456,01	45 52 59·5	231·5	—11 ·0011,85
	San Vito.....	51 ·0170,48			—11 ·6920,64
7	Vaux	50 ·9882,65	45 52 19·9	124·1	+ 0 ·8178,06
	Royères	50 ·9499,50			+ 0 ·5095,84
8	Limoges	50 ·9222,16	45 51 35·0	214·0	+ 1 ·2001,77
	Vaux	50 ·9882,50			+ 0 ·8178,06
9	Rouillac	50 ·8917,68	45 51 12·2	365·7	+ 2 ·6773,40
	Negret	51 ·0046,33			+ 2 ·0469,12
10	Bossivre	50 ·9452,77	45 50 59·0	7·5	— 2 ·2715,74
	Limonest ...	50 ·9429,77			— 2 ·7139,21
11	Mur	50 ·8497,81	45 50 51·2	595·9	— 1 ·0147,24
	Montoncelle .	51 ·0337,09			— 1 ·5083,98
12	San Vito... ..	51 ·0170,50	45 50 33·9	522·7	—11 ·6920,98
	Acquileja ...	50 ·8557,34			—12 ·2631,81
13	Puy Cogneux	50 ·7304,50	45 49 57·8	1262·4	+ 1 ·6127,00
	Sauvagnac ...	51 ·1200,66			+ 0 ·9924,50
14	Royères	50 ·9499,50	45 48 13·6	368·5	+ 0 ·5095,84
	Puy-de-Gué	50 ·8362,08			+ 0 ·1274,35
15	Burie	50 ·8515,71	45 47 4·2	130·2	+ 3 ·0791,40
	Rouillac	50 ·8917,54			+ 2 ·6773,40
16	Marennes ...	50 ·9118,63	45 46 57·7	273·4	+ 3 ·8275,47
	La Ferlanderie	50 ·8274,80			+ 3 ·3554,00
17	Puy-de-Dôme	50 ·8578,72	45 46 6·4	26·2	— 0 ·6973,12
	Mur	50 ·8497,74			— 1 ·0147,13
18	Hermant ...	50 ·8366,50	45 45 46·6	68·7	— 0 ·2572,46
	Puy-de-Dôme	50 ·8578·72			— 0 ·6973,12

MEAN PARALLEL.

No.	Amplitude of Arc.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
				Measured.	Computed.	
	°	Toises.	Meters.	Toises.	Toises.	Toises.
1	0·4573890	3,267,926	35,421·2	39,611·7	39,612·6	—0·9
2	0·4916583	3,267,900	40,003·0	39,648·2	39,649·6	—1·4
3	0·6834564	3,267,872	53,437·8	39,682·8	39,682·9	—0·1
4	0·2951946	3,267,863	25,152·0	39,693·3	39,694·7	—1·4
5	0·6868494	3,267,860	54,027·5	39,697·4	39,698·1	—0·7
6	0·6217911	3,267,856	48,783·8	39,703·9	39,703·4	+0·5
7	0·2773998	3,267,849	21,870·8	39,710·2	39,711·1	—0·9
8	0·3441339	3,267,841	27,522·4	39,717·3	39,720·7	—3·4
9	0·5673852	3,267,838	45,480·2	39,722·8	39,724·3	—1·5
10	0·3981123	3,267,835	30,916·0	39,726·4	39,726·8	—0·4
11	0·4443066	3,267,835	39,101·3	39,725·8	39,728·5	—2·7
12	0·5139747	3,267,833	43,055·4	39,729·8	39,731·9	—2·1
13	0·5582250	3,267,826	58,299·7	39,739·1	39,739·0	+0·1
14	0·3439341	3,267,809	29,050·4	39,758·2	39,759·5	—1·3
15	0·3616200	3,267,797	28,400·0	39,772·5	39,773·1	—0·6
16	0·4249323	3,267,796	34,098·7	39,773·7	39,774·3	—0·6
17	0·2856564	3,267,788	22,229·5	39,784·3	39,784·4	—0·1
18	0.3960594	3,267,784	30,877·1	39,788·8	39,788·4	+0·4

No.	Stations.	Latitudes.	Mean Latitude.	Difference in Latitude.	Longitudes.
			° ′ ″	″	
19	La Ferlanderie . Burie	50g·8274,80 50 ·8515,71	45 45 20·6	78·1	+ 3g·3554,00 + 3 ·0791,40
20	Puy-de-Gué...... Hermant.........	50 ·3862,20 50 ·8366,36	45 45 10·0	1·3	+ 0 ·1274,37 − 0 ·2572,41
21	Sablonceaux ... La Ferlanderie...	50 ·7970,45 50 ·8274,80	45 43 51·7	98·6	+ 3 ·5729,21 + 3 ·3554,00
22	Acquileja......... Caorle	50 ·8557,38 50 ·6658,15	45 41 4·9	615·3	−12 ·2631,95 −11 ·7283,41
23	Cordouan......... La Ferlanderie...	50 ·6509,59 50 ·8274,80	45 39 54·5	572·0	+ 3 ·9015,50 + 3 ·3555,54
24	Cordouan......... Sablonceaux ...	50 ·6508,65 50 ·7970,45	45 39 5·6	473·6	+ 3 ·9013,57 + 3 ·5729,21
25	Saint-André ... Chandieu.........	50 ·7087,69 50 ·7055,24	45 38 11·2	10·5	− 2 ·5077,13 − 2 ·9622,29
26	Montceau ...... Chandieu.........	50 ·6565,09 50 ·7055,24	45 36 46·5	158·8	− 3 ·3669,29 − 2 ·9622,30
27	Puy Cogneux ... La Condamine...	50 ·7304,39 50 ·6256,92	45 36 37·0	339·4	+ 1 ·6127,00 + 1 ·1973,03
28	Brisebart......... Puy Cogneux ...	50 ·6047,70 50 ·7304,39	45 36 3·0	407·2	+ 2 ·2877,40 + 1 ·6126,55
29	Pierre-sur-Autre Sury-le-Comtal .	50 ·7248,33 50 ·5984,32	45 35 43·7	409·5	− 1 ·6365,22 − 2 ·0518,22
30	La Condamine... Saint-Gilles......	50 ·6257,00 50 ·6880,89	45 35 28·3	202·1	+ 1 ·1973,00 + 0 ·7650,00
31	Sury-le-Comtal . Saint-André ...	50 ·5984,32 50 ·7087,69	45 35 17·7	357·5	− 2 ·0518,22 − 2 ·5077,13
32	Isson .... Pierre-sur-Autre	50 ·5754,21 50 ·7248,33	45 35 6·4	484·1	− 0 ·9114,00 − 1 ·6365,22
33	Cordouan......... Epargnes.........	50 ·6509,17 50 ·6014,60	45 33 48·5	160·2	+ 3 ·9013,63 + 3 ·4899,71
34	Chadenac......... Nonaville	50 ·6049,60 50 ·6037,77	45 32 38·2	3·8	+ 3 ·1211,10 + 2 ·6803,14
35	Nonaville ...... Brisebart.........	50 ·6037,80 50 ·6047,60	45 32 37·9	3·2	+ 2 ·6803,14 + 2 ·2877,46
36	Epargnes......... Chadenac.........	50 ·6014,60 50 ·6049,99	45 32 34·5	11·5	+ 3 ·4899,71 + 3 ·1211,06
37	Monte Stanig ... Pirano	50 ·5934,67 50 ·5885,06	45 31 54·8	16·1	−12 ·9360,12 −12 ·4833,45

No.	Amplitude of Arc.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
				Measured.	Computed.	
		Toises.	Meters.	Toises.	Toises.	Toises.
19	0·2486340	3,267,780	19,489·6	39,792·8	39,793·5	—0·7
20	0·3462102	3,267,778	26,931·4	39,796·3	39,795·4	+0·9
21	0·1957689	3,267,765	15,535·8	39,810·6	39,810·9	—0.3
22	0·4813686	3,267,738	42,028·0	39,841·5	39,843·5	—2·0
23	0·4913964	3,267,726	42,162·3	39,857·0	39,857·3	—0·3
24	0·2955924	3,267,718	27,283·9	39,864·7	39,866·8	—2·1
25	0·4090644	3,267,709	31,888·5	39,877·9	39,877·5	+0·4
26	0·3642291	3,267,695	28,823·8	39,094·5	39,894·0	+0·5
27	0·3738573	3,267,693	30,981·5	39,895·6	39,895·9	—0·3
28	0·6075765	3,267,687	49,029·2	39,901·9	39,902·6	—0·7
29	0·3737700	3,267,684	31,779·7	39,905·0	39,906·3	—1·3
30	0·3890700	3,267,682	30,987·3	39,909·6	39,909·3	+0·3
31	0·4103019	3,267,680	33,859·4	39,910·6	39,911·4	—0·8
32	0·6526098	3,267,678	53,064·9	39,913·0	39,913·6	—0·6
33	0·3702528	3,267,665	29,318·5	39,928·5	39,928·8	—0·3
34	0·3967164	3,267,653	30,975·9	39,944·1	39,942·7	+1·4
35	0·3533112	3,267,653	27,586·3	39,943·3	39,942·8	+0·5
36	0·3319785	3,267,653	25,923·4	39,944·2	39,943·4	+0·8
37	0·4074003	3,267,646	31,820·2	39,951·9	39,951·0	+0·9

No.	Stations.	Latitudes.	Mean Latitude.	Difference in Latitude.	Longitudes.
			° ′ ″	″	
38	Montceau......	50g·6565,09	45 31 27·2	455·7	— 3g·3669,29
	Le Granier ...	50 ·5158,54			— 3 ·9879,45
39	Belle Achat...	50 ·6011,05	45 31 3·7	167·8	— 4 ·5216,85
	Mt. Jouvet ...	50 ·5493,23			— 4 ·7816,82
40	Venice.........	50 ·4822,87	45 30 59·9	594·6	—11 ·1166,59
	Caorle	50 ·6658,15			—11 ·7283,41
41	Caorle	50 ·6658,15	45 30 18·3	677·7	—11 ·7283,41
	Buje............	50 ·4566,21			—12 ·5805,13
42	Granier	50 ·5158,54	45 30 9·9	276·3	— 3 ·9879,45
	Belle Achat...	50 ·6011,14			— 4 ·5216,69
43	Colognola......	50 ·4813,00	45 25 39·7	39·5	— 9 ·8417,83
	San Giovanni	50 ·4691,10			—10 ·2126,95
44	Padua	50 ·4398,77	45 24 53·9	137·4	—10 ·6062,70
	Venice.........	50 ·4822,86			—11 ·1166,64
45	Milan	50 ·5154,88	45 24 48·4	363·6	— 7 ·6187,19
	Crema	50 ·4032,62			— 8 ·1695,82
46	San Giovanni	50 ·4691,11	45 24 32·6	94·7	—10 ·2127,00
	Padua	50 ·4398,74			—10 ·6062,64
47	Solferino	50 ·4129,14	45 24 26·1	256·5	— 9 ·1435,43
	Verona.........	50 ·4920,76			— 9 ·6258,36
48	Vigevano......	50 ·3519,06	45 23 25·2	530·0	— 7 ·2468,24
	Milan	50 ·5154,88			— 7 ·6187,19
49	Mas[illegible]	50 ·3376,89	45 22 34·6	521·1	— 6 ·2261,04
	Novara.........	50 ·4985,09			— 6 ·9830,33
50	Verola Nuova	50 ·3608,27	45 20 53·5	168·7	— 8 ·6028,17
	Solferino	50 ·4129,14			— 9 ·1435,43
51	Buje............	50· 4566,21	45 20 53·1	452·7	—12 ·5805,11
	Mte. Maggiore	50 ·3168,88			—13 ·1872,86
52	Crema	50 ·4032,52	45 20 37·8	137·5	— 8 ·1695,73
	Verola Nuova	50 ·3608,27			— 8 ·6028,17
53	Monte Cero...	50 ·2835,70	45 19 32·0	506·4	—10 ·3711,59
	Padua	50 ·4398,77			—10 ·6062,70
54	Mte. Maggiore	50 ·3168,88	45 18 21·6	149·7	—13 ·1872,86
	Fiume	50 ·3630,71			—13 ·4552,46
55	Crema	50 ·4032,52	45 14 53·6	825·8	— 8 ·1695,73
	Cremona	50 ·1483,70			— 8 ·5447,73
56	Monte Cero...	50 ·2835,70	45 14 10·5	136·6	—10 ·3711,59
	Chioggia	50 ·2414,15			—11 .0477,33

No.	Amplitude of Arc.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
				Measured.	Computed.	
	°	Toises.	Meters.	Toises.	Toises.	Toises.
38	0·5589144	3,267,642	45,863·2	39,954·5	39,956·4	—1.9
39	0·2339973	3,267,638	18,998·2	39,960·7	39,961·0	—0·3
40	0·5505138	3,267,637	46,757·0	39,960·4	39,961·8	—1·4
41	0.7669548	3,267,630	63,472·4	39,970·7	39,969·9	+0·8
42	0·4803516	3,267,629	38,489·7	39,972·1	39,971·6	+0·5
43	0·3338208	3,267,584	26,146·4	40,025·4	40,024·2	+1·2
44	0·4593546	3,267,576	36,197·2	40,034·5	40,033·2	+1·3
45	0·4957767	3,267,575	40,390·2	40,035·0	40,034·2	+0·8
46	0·3542076	3,267,573	27,876·0	40,038·8	40,037·3	+1·5
47	0·4340637	3,267,571	34,883·5	40,039·2	40,038·5	+0·7
48	0·3347055	3,267,561	30,892·0	40,047·6	40,050·4	—2·8
49	0·6812361	3,267,554	55,719·6	40,060·4	40,060·3	+0·1
50	0·4866534	3,267,536	38,483·0	40,081·6	40,080·1	+1·5
51	0·5460975	3,267,536	45,011·3	40,081·3	40,080·1	+1·2
52	0·3899196	3,267,534	30,845·9	40,083·5	40,083·0	+0·5
53	0·2115999	3,267,522	22,791·3	40,090·7	40,095·8	—5·1
54	0·2411640	3,267,511	19,464·9	40,109·3	40,109·5	—0·2
55	0·3376800	3,267,476	36,772·3	40,143·1	40,150·0	—6·9
56	0·6089166	3,267,469	47,988·0	40,167·2	40,158·3	+8·9

No.	Stations.	Latitudes.	Mean Latitude.	Difference in Latitude.	Longitudes.
			° ′ ″	″	
57	Cerea	50g·2167,34	45 13 30·5	216·5	− 9g·8640,05
	Monte Cero...	50 ·2835,70			−10 ·3711,59
58	Crea............	50 ·1054,73	45 12 21·0	798·4	− 6 ·5972,84
	Vigevano......	50 ·3519,06			− 7 ·2468,24
59	Rochemelon...	50 ·2256,40	45 11 38·3	65·6	− 5 ·2684,45
	Mt. Cevrari ...	50 ·2053,90			− 5 ·5554,28
60	Ambin.........	50 ·1738,24	45 10 47·1	167·9	− 5 ·0539,25
	Rochemelon...	50 ·2256,40			− 5 ·2684,45
61	Pavia	50 ·2052,24	45 10 47·0	35·9	− 7 ·5755,36
	SanColombano	50 ·1941,34			− 7 ·9276,17
62	Mantua	50 ·1771,37	45 10 38·1	128·3	− 9 ·4014,55
	Cerea	50 ·2167,34			− 9 ·8640,05
63	Acqua Negra	50 ·1799,03	45 9 38·4	7·0	− 8 ·9948,28
	Mantua	50 ·1771,37			− 9 ·4014,55
64	SanColombano	50 ·1941,34	45 9 14·8	148·3	− 7 ·9276,17
	Cremona	50 ·1483,71			− 8 ·5447,62
65	Cremona	50 ·1483,71	45 8 51·8	102·1	− 8 ·5447,62
	Acqua Negra	50 ·1799,03			− 8 ·9948,28
66	Mt. Tabor ...	50 ·1262,54	45 8 6·1	154·1	− 4 ·6978,17
	Ambin.........	50 ·1738,26			− 5 ·0539,37
67	Mt. Cevrari ...	50 ·2053,90	45 7 57·4	376·2	− 5 ·5554,28
	Superga	50 ·0892,91			− 6 ·0358,65
68	Superga	50 ·0892,91	45 5 15·5	52·4	− 6 ·0358,65
	Crea............	50 ·1054,73			− 6 ·5972,84
69	Albergian ..	50 ·0077,65	44 59 25·0	120·3	− 5 ·1744,24
	Freidour	49 ·9706,38			− 5 ·5218,75
70	Chaberton ...	49 ·9605,43	44 59 8·7	153·0	− 4 ·9058,61
	Albergian ...	50 ·0077,66			− 5 ·1744,27

he "Correction," applied in the Type of Calcula-ı, alone requires explanation. The "measured ıgths" of the sides of triangles and the correspond-ing amplitudes of arcs are computed on the present erroneous theory of the Earth's shape and dimen-

No.	Amplitude of Arc.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
				Measured.	Computed.	
	°	Toises.	Meters.	Toises.	Toises.	Toises.
57	0·4564386	3,267,463	36,457·0	40,168·1	40,166·1	+2·0
58	0·5845860	3,267,451	52,111·9	40,178·6	40,179·6	−1·0
59	0·2582847	3,267,444	20,392·2	40,190·2	40,187·9	+2·3
60	0·1930680	3,267,436	16,032·1	40,198.7	40,197·9	+0·8
61	0·3168729	3,267,436	24,925·5	40,201·0	40,197·9	+3·1
62	0·4162950	3,267,434	32,952·6	40,200·9	40,199·6	+1·3
63	0·3659643	3,267,424	28,767·9	40,212·7	40,211·2	+1·5
64	0·5554305	3,267,420	43,904·5	40,217·3	40,215·7	+1·6
65	0·4050594	3,267,416	32,003·7	40,222·4	40,220·2	+2·2
66	0·3205080	3,267,409	25,650·6	40,231·2	40,229·1	+2·1
67	0·4323933	3,267,408	35,933·9	40,232·5	40,230·8	+1·7
68	0·5052771	3,267,380	39,802·3	40,264·9	40,262·1	+2·8
69	0·3127059	3,267,322	24,932·3	40,332·9	30,330·0	+2·9
70	0·2417094	3,267,319	19,634·6	40,335·6	40,333·1	+2·5

sions. Whether the error assumes the geodetic or the astronomical form, or both, is a matter of indifference; in either case it represents a certain linear value, by which the astronomical calculation has to be corrected and reduced to those results, which would have been obtained, had the true figure and the true dimensions of the Earth been made the basis of a correct computation. Generally, the value of these corrections, as applied to the astronomical results, is expressed by the combined value of the differences of columns VI. and IX. of our table page 119. While the arc of a parallel is taken too great by the corresponding difference of column IX., it is taken too small by the difference of column VI., multiplied with the cosine of the latitude. But these differences somewhat vary, according to the amount of polar depression adopted, and the tendency and complication of the formulas, made use of, by individual astronomers, in each special case. As regards the great arc of the mean parallel, the ellipticity assumed is $\frac{1}{309}$; the formulas employed being those of Puissant. The sections, calculated by me, are included between 46° and 45° of latitude: the mean being 45° 30′. For this latitude, I find the error of linear extent in a degree, computed on the erroneous theory, to be—

$$
\begin{array}{rlr}
 & & -125^{t}\cdot 6 \\
+\ 12^{t}\cdot 2 = & \text{log. } 1\cdot 0864 & \\
\text{Cos. } 45^\circ\ 30' = & \text{,, } \ \ 9\cdot 8457 & \\
\hline
 & \text{log. } 0\cdot 9321 = & +\ \ 8\cdot 6 \\
\hline
 & & -\ 117^{t}\cdot 0\,;
\end{array}
$$

and the variation for 30′=1^{t}·5. To save, therefore, what would here be useless trouble, I have made the correction

$$\text{for } 46^\circ = 115^t\cdot 5$$
$$,, \ 45^\circ = 118\cdot 5,$$

and applied it, in an arithmetical proportion, to the arcs calculated. The error of this process, being very small, cannot possibly affect the results in a sensible manner.

I might be content to let this subject rest here, *because, the polar elongation of the Earth, the amount of its ellipticity, and the proportions of its meridional dimensions having been fully established, the correctness of the linear dimensions of parallel degrees, as computed by me, follows from those of the corresponding meridian degrees* AS A NECESSARY CONSEQUENCE; unless, indeed, the formula $l = r \cos. \phi . \frac{\pi}{180}$, and, with it, the empirical relation of the ellipse to the circle, on which it rests, be impugned. I will, however, fully explain the astronomical error presently, when I come to speak of the arc between Greenwich and Valentia.

Independently, then, of theory, nothing could place in a more brilliant light the admirable manner, in which this great and difficult geodetic operation, which you "are not inclined to rate very high," * has

* "Figure of the Earth," p. 217.

been conducted and carried out, than the comparative results of our table. The addition of the differences between the measured and the computed distances, shows the sum to be

$$\begin{array}{r} +\ 52^{t}\!\cdot\!5 \\ -\ 47\cdot1 \\ \hline 70)\ +\ \ 5^{t}\!\cdot\!4 \end{array}$$

; and, therefore, the mean difference = + 0·ᵗ078, being *six inches* in a degree.*

Comparing the differences, relating to the greater sections, into which the entire arc has been divided, we have for the approximate distances—

* In the Arc from Marennes to Padua, you make the "error in measure" = + 789 English feet. This is incorrect. For, taking the measured length of the arc, as you do, at 3,316,976 English feet, and the French meter at 3·280899 English feet, that length would be equal to 1,010,997 French meters. But the measured length in French meters is 1,011,101 (Arago, "Astron. Pop.," tom. iii., p. 339), making a difference of 104 French meters, or 341 of our feet, by which you take the "measured length" of the arc short of the truth. Consequently, your "error in measure," instead of 789 feet, should be 789 + 341 = 1,130 feet. Admiral Smyth and Prof. Grant (Arago's "Popul. Astron.," tom. ii., p. 212), correctly state the measured length of the arc to be 3,317,320 English feet, or 344 feet in excess of your number. According to Prof. Encke ("Berl. Jahrb. für 1852," p. 359), a degree of the parallel of 45° 43′ 12″ of latitude (the mean latitude of the arc in question) should measure 39939·512ᵗ, or, taking the meter at 0·513074ᵗ (Arago, "Astron. Pop.," tom. iv., p. 77), 77843·73 meters; the whole arc of 12° 59′ 3″·8, consequently, 1,010,753 meters. It *does* measure, as already observed, 1,011,101 meters: difference, 348 meters or 1,142 English feet.

Between Marennes and Saint-Preuil :—

Mean $-0^{t}\cdot300$	No. 15 $-\ 0^{t}\cdot6$	No. 21 $-\ 0^{t}\cdot3$	No. 33 $-\ 0^{t}\cdot3$	$-\ 4^{t}\cdot9$
	„ 16 $-\ 0\cdot6$	„ 23 $-\ 0\cdot3$	„ 34 $+\ 1\cdot4$	$+\ 2\cdot2$
	„ 19 $-\ 0\cdot7$	„ 24 $-\ 2\cdot1$	" 36 $+\ 0\cdot8$	
				$-\ 2^{t}\cdot7$

Between Saint-Preuil and Sauvagnac :—

Mean $-0^{t}\cdot178$	No. 1 $-\ 0^{t}\cdot9$	No. 15 $-\ 0^{t}\cdot6$	No. 30 $+\ 0^{t}\cdot3$	$-\ 3^{t}\cdot9$
	„ 2 $-\ 1\cdot4$	„ 27 $-\ 0\cdot3$	„ 34 $+\ 1\cdot4$	$+\ 2\cdot3$
	„ 13 $+\ 0\cdot1$	„ 28 $-\ 0\cdot7$	„ 35 $+\ 0\cdot5$	
				$-\ 1^{t}\cdot6$

Between Sauvagnac and Isson :—

Mean $-0^{t}\cdot117$	No. 7 $-\ 0^{t}\cdot9$	No. 18 $+\ 0\cdot4$	$-\ 2^{t}\cdot3$
	„ 14 $-\ 1\cdot3$	„ 20 $+\ 0\cdot9$	$+\ 1\cdot6$
	„ 17 $-\ 0\cdot1$	„ 30 $+\ 0\cdot3$	
			$-\ 0^{t}\cdot7$

Between Isson and Geneva :—

Mean $-0^{t}\cdot167$	No. 3 $-\ 0^{t}\cdot1$	No. 25 $+\ 0^{t}\cdot4$	No. 31 $-\ 0^{t}\cdot8$	$-\ 2^{t}\cdot9$
	„ 5 $-\ 0\cdot7$	„ 26 $+\ 0\cdot5$	„ 32 $-\ 0\cdot6$	$+\ 1\cdot4$
	„ 10 $-\ 0\cdot4$	„ 29 $-\ 1\cdot3$	„ 42 $+\ 0\cdot5$	
				$-\ 1^{t}\cdot5$

Between Geneva and Milan :—

Mean $+\ 0^{t}\cdot500$	No. 39 $-\ 0^{t}\cdot3$	No. 49 $+\ 0^{t}\cdot1$	No. 60 $+\ 0^{t}\cdot8$	$+\ 8\cdot6$
	„ 42 $+\ 0\cdot5$	„ 58 $-\ 1\cdot0$	„ 66 $+\ 2\cdot1$	$-\ 4\cdot1$
	„ 48 $-\ 2\cdot8$	„ 59 $+\ 2\cdot3$	„ 68 $+\ 2\cdot8$	
				$+\ 4^{t}\cdot5$

Between Milan and Padua :—

Mean $+\ 0^{t}\cdot100$	No. 43 $+\ 1^{t}\cdot2$	No. 50 $+\ 1^{t}\cdot5$	No. 57 $+\ 2^{t}\cdot0$	$+\ 13^{t}\cdot2$
	„ 45 $+\ 0\cdot8$	„ 52 $+\ 0\cdot5$	„ 62 $+\ 1\cdot3$	$-\ 12\cdot0$
	„ 46 $+\ 1\cdot5$	„ 53 $-\ 5\cdot1$	„ 63 $+\ 1\cdot5$	
	„ 47 $+\ 0\cdot7$	„ 55 $-\ 6\cdot9$	„ 65 $+\ 2\cdot2$	
				$+\ 1^{t}\cdot2$

Between Padua and Fiume :—

Mean -0^{t}·111	No. 6 + 0^{t}·5	No. 37 + 0^{t}·9	No. 44 + 1^{t}·3	− 5^{t}·7
	„ 12 − 2 ·1	„ 40 − 1 ·4	„ 51 + 1 ·2	+ 4 ·7
	„ 22 − 2 ·0	„ 41 + 0 ·8	„ 54 − 0 ·2	− 1^{t}·0

Hence, the mean differences of these single arcs, compared with their differences from their general mean, as given by Arago, are :—

	According to	
	the new theory.	the present mode of calculation.*
Marennes and Saint-Preuil	− 0^{t}·30	+ 46^{t}·10
St. Preuil and Sauvagnac	− 0 ·18	− 50 ·12
Sauvagnac and Isson	− 0 ·12	− 52 ·89
Isson and Geneva	− 0 ·17	+ 18 ·72
Geneva and Milan	+ 0 ·50	− 12 ·49
Milan and Padua	+ 0 ·10	− 39 ·90
Padua and Fiume	− 0 ·11	+ 84 ·38

The difference for the arc from Marennes to Padua, comprising about thirteen degrees, and, according to you, " subject to the accumulated errors of six independent determinations in difficult circumstances," † amounts, at the mean value of + 0^{t}·078 in a degree, *just to one toise*, or not quite six feet and a half.

The next table (pp. 198, 199) contains the particulars of the four principal sections, constituting the

* " Astronomie Populaire," vol. iii., p. 339.

† " Figure of the Earth," p. 218.

arc across the mouth of the Rhone, measured by Lacaille and Cassini de Thury.

The method, employed to determine the amplitude of this arc, involves an error in longitude similar to the error in latitude, involved in the method of determining the amplitude of the Swedish arc No. 14, made use of by Maupertuis. I have inserted, however, in the table, the *given* longitudes, and applied the correction in the logarithmic form of —0·0012081 to the logarithm of the "measured length of a degree," as directly resulting from the tabular elements. The measured base coincides very nearly with the parallel; and, regarding it geometrically as a chord subtending the measured arc, the distances have been calculated by the formulas of plane trigonometry.

The "measured lengths of arc," obtained by myself, differ but by the fraction of a toise from the corresponding values, computed by Von Zach* from the corrected data, given by Cassini in his "Description Géométrique de la France." In this case, the further correction for curvature ($-7^{t}\cdot4$) has to be applied. The "measured lengths of a degree," thus corrected, should directly correspond to the "computed lengths." And so they are found to do.

* "Monatl. Corresp.," vol. xiii., p. 320.

FRENCH ARC ACROSS

No.	Stations.	Latitudes.	Mean Latitude.	Differences in Latitude.	Longitudes.*
		° ′ ″	° ′ ″	″	° ′ ″
71	Houpies	43 42 42·3	43 44 48·0	251·5	0 35 52·1
	Calvisson	43 46 53·8			1 23 49·9
72	St. Victoire ...	43 31 49·0	43 29 52·2	293·6	0 0 0·0
	Lebres	43 26 55·4			0 32 19·2
73	Lebres	43 26 55·4	43 27 0·1	9·4	0 32 19·2
	Les Stes. Maries	43 27 4·8			1 9 10·9
74	Les Stes. Maries	43 27 4·8	43 25 36·7	176·3	1 9 10·9
	Cette	43 24 8·5			1 53 49·0

The sum of the differences being

$$\begin{array}{r} +\ 14^{t}\!\cdot\!2 \\ -\ 12\ \cdot\!8 \\ \hline \end{array}$$

4) + $1^{t}\cdot4$, the mean difference, consequently, is = + $0^{t}\cdot35$ only; and it will be remarked, moreover, that the two principal differences would be made to vanish by a very slight alteration in the latitude of Lebres. The result, therefore, is a very satisfactory one, and speaks highly in favor of the entire operation. You consider it only "pretty good," and make the error of measure = + 2,159 feet, being equal to $181^{t}\cdot8$ in a degree.

* The longitudes, it will be noticed, are reckoned from St. Victoire. Both the longitudes and the amplitudes of arc, as here given, are the uncorrected values.

THE MOUTH OF THE RHONE.						
No.	Amplitude of Arc.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
				Measured.	Computed.	
	′ ″		Toises.	Toises.	Toises.	Toises.
71	47 57·8	3,266,581	33,259·1	41,184·1	41,186·1	− 2·0
72	32 19·2	3,266,432	22,815·6	41,344·3	41,355·1	−10·8
73	36 51·7	3,266,403	25,511·2	41,400·2	41,387·5	+12·7
74	44 38·1	3,266,393	31,016·6	41,401·7	41,403·2	+ 1·5

It will be instructive as well as interesting, to compare with the elements of the preceding arc those of the only four similar sections, approaching to true arcs of parallels, which are included in the arc between Greenwich and Valentia, determined chronometrically by yourself, geodetically by the late General Colby. The next table exhibits the results. For reasons, which will presently appear, I give in the first instance the values of the "measured length of a degree," such as they immediately follow from your "amplitude of arc" and "measured length of arc," together with the corresponding differences. The true differences will be determined afterwards.

ENGLISH ARCS BETWEEN					
No.	Stations.	Latitudes.	Mean Latitude.	Differences in Latitude.	Longitudes.
		° ′ ″	° ′ ″	″	° ′ ″
75	Forth .... Knockanafrion	52 18 57·2 52 17 19·4	52 18 8·3	97·8	6 33 42·3 7 34 53·9
76	Knockanafrion Bartregaun ...	52 17 19·4 52 12 24.4	52 14 51·9	295·0	7 34 53·9 9 49 45·5
77	Mendip......... Dunkerry	51 13 4·9 51 9 44·2	51 11 24·5	200·7	2 32 36·7 3 35 8·4
78	Butser .... Wingreen	50 58 38·2 50 59 4·7	50 58 51·5	26·5	0 58 43·8 2 6 25·9

As the general result of your "Determination of the Longitude of Valentia by the transmission of Chronometers,"* you state,—"I think we are entitled to conclude that no improvement can be made in the assumed elements for the Earth's figure"—those of your "investigation published in the article 'Figure of the Earth' in the 'Encyclopædia Metropolitana,'" —"so far as they apply to the circumference of a parallel or to the measure of 1″ on the arc perpendicular to the meridian . . . And we may, therefore, lay down *as one of the most certain data for the determination of the Earth's figure*, that in latitude 51° 40′ the complement of the logarithm of the number of

* Appendix to the Greenwich Observations for 1845.

GREENWICH AND VALENTIA.						
No.	Amplitude of Arc.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
				Measured-Computed.	Computed.	
	° ′ ″	Toises.	Log. of English Feet.	Toises.	Toises.	Toises.
75	1 1 11·6	3,271,639	5·3587705	34,993·3	34,916·9	+76·4
76	2 14 51·6	3,271,607	5·7028291	35,035·5	34,959·6	+75·9
77	1 2 31·7	3,270,996	5·3799736	35,863·8	35,780·3	+83·5
78	1 7 41·1	3,270,872	5·4148029	36,025·9	35,941·0	+84·9

feet in 1″ for an arc perpendicular to the meridian is 7·9928932, or the length of 1″ in an arc perpendicular to the meridian in latitude 51° 40′ is 101·6499 feet."

There is now nothing more certain, than the error of these data. They give for the length of a degree perpendicular to the meridian 57226^{t}·7. Professor Encke finds it = 57226^{t}·1. The agreement is very close; but Professor Encke himself remarks upon it,* "Such an accordance was to be expected, because Mr. Airy's data are not directly deduced from observation; he has only shown that his assumed elements very nearly agree with observation, and that

* "Berliner Jahrbuch für 1852," pp. 341-2.

his data follow from his assumption." The real point, however, to which I have to call attention, is, that, while according to both Professor Encke and yourself, in latitude 51° 40′,

a degree, perpendicular to the meridian, measures		57,226t·5,	nearly,
and a meridian degree	„	57,078 ·6	„
the former exceeding the latter by ...		147t·9,	

the positive proof has been furnished by me, that *both degrees are identical*, and that their true common length is 57,094t·4, being that of a meridian degree in the latitude in question.

Let us now see, how this bears on the geodetic part of your determination. After describing the method used, to compute from the results of the triangulation the corresponding astronomical longitudes and latitudes, you go on to say, "The next point was to determine on the *spheroidal elements which should be used.* It has been the practice for some years to employ in the geodetic calculations made in the Ordnance Map-office, under Colonel Colby's direction, elements which I obtained by an investigation shed in the article 'Figure of the Earth' in the yclopædia Metropolitana.' These elements are—

Polar semi-axis	20,853,810 feet
Equatorial	20,923,713 „

and the tables computed from them, and *employed* *in the geodetic calculations of this section, are the following:—*

"COMPLEMENT OF THE LOGARITHM OF THE NUMBER OF FEET IN 1″.

Latitude.	For Arc perpendicular to the Meridian.	For Arc in the Meridian.
50° 30′	7·9929221	7·9941006
40	29180	40882
50	29137	40754
51 0	29096	40631
10	29055	40508
20	29014	40384
30	28973	40262
40	28932	40138
50	28890	40013
52 0	28849	39889
10	28808	39767
20	28767	39644
30	28727	39523
40	28686	39400
50	28645	39277
53 0	28604	39154."

In other words, *the direct results of the geodetic measurement are first adapted to the theoretical shape of the Earth; and thereupon, from the accordance, consequent on that adaptation, the theoretical shape of the Earth is concluded to be its true shape.* We have to undo again, therefore, the theoretical part of the process, in order to arrive at the immediate results of the geodetic operation. This is effected by taking, for any given mean latitude, the corresponding difference between the complement of the logarithms of the number of feet in 1″ for "arc perpendicular to the meridian," and for "arc in the meridian," and deducting it from the logarithm of the measured length of a degree of the parallel of the same mean latitude, as it results from the "Logarithm of distance in feet," given by General Colby. For, the simple fact is, that astronomers, in order to obtain the theoretical length of a degree of the parallel *f. i.* of latitude 51° 40′, should have multiplied the theoretical length of a meridian degree in that latitude, according to Professor Encke's tables = 57,078^{t}·6 with the cosine of the latitude; when they would have found it = 35,402^{t}·2, differing only by −9^{t}·8 from the truth. Instead of which, *for purely theoretical reasons*, they extend the true dimensions of the true meridian degree, under the name of "a degree perpendicular to the meridian," from 57,078^{t}·6 to the theoretical and purely imaginary dimensions of 57,226^{t}·1; and, multiplying this length with the cosine of the latitude, the length of the corresponding degree of the parallel is found to be 35493^{t}·6, which agrees exactly with Professor Encke's tables. Hence, *the measured length, as directly resulting from triangulation*, here assumed = astronomical theoretical length of meridian degree × cos. 51° 40′, or 35,402^{t}·2 .... = log. 4·5490299 has to be similarly extended; and this, as I have shown, is done by adding the difference of your tabular logarithms, p. 231, for

51° 40′ .... = log. 7·9928932
„ 7·9940138
log. 0·0011206

35,493ᵗ·6 = log. 4·5501505,

which gives the required length to the fraction of a toise. This part of the process, then, and of the astronomical error, I think, has been clearly and sufficiently explained. It leaves me but to add, that there is another correction to be applied to the "measured length of arc," as given by you, namely, the correction for difference of curvature, which is here additive, because that length has not been deduced from a base in the parallel, but from a meridian base in latitude 55° 5′ 50″.

We next have to inquire into the true amplitude of the measured arc; not with reference to all the details of your laborious operation, but with reference to the principle, on which it rests. The measurement was effected *chronometrically*, or by *time*. But already Saint Augustinus, in answer to his own question, "*Quid est tempus?*" confessed, "*Si nemo ex me quæreat, scio; si quærenti explicare velim, nescio*": and not saints alone, astronomers also have experienced, and to this day continue, to experience the same difficulty. Of one thing only the latter are certain, namely, that *sidereal* time is *true* time. And in this they are mistaken. Even the length of the sidereal day is subject to certain small fluctuations, unknown to the present astronomical theory.

It is a somewhat striking feature of your extensive memoir that, although you describe * the very stones, which were lying in the path of the men, who carried the chronometers between the Valentia side of the ferry and the station of Feagh Main, you nowhere state whether the amplitude of the arc was determined in sidereal, solar, or any other time, but leave this fundamental point of your inquiry to be inferred. It is actually sidereal time you use, *i.e.*, you have, as is the custom of modern astronomy, chosen the point of the vernal equinox, considered in the light of a *star*, as a mark, to mark, by the return of the same terrestrial meridian to it, a completed revolution of the Earth about itself. Hence, the conditions of the accuracy of the chronometrical determinations are, *in principle*, that, independently of the Earth's revolution about the Sun, the apparent relative positions of the Earth and the stars in space be absolutely fixed; that, at the extremities of the arc and at the

* "Determination," p. xi.

intermediate stations, the transits of certain stars be correctly observed; that the transit-clocks show time correctly, and go, as well as the stationary and the travelling chronometers, at a perfectly uniform rate; and that this rate be precisely that of twenty-four hours during one of the Earth's sidereal revolutions about itself.

The latter conditions are not, and cannot be, fulfilled in reality. On comparing *f. i.* the first observations of transits of the same star, made at Valentia and Greenwich on the same day, we find—

	Clock time of Transit corrected.	Seconds of Star's apparent R. A.	Clock slow.
1844. August 17.			
Valentia, δ Ursæ Minoris, W.	$18^h\,20^m\,54^s{\cdot}80$	$35^s{\cdot}99$	$+\ 41^s{\cdot}19$
	41 ·19		
	$18^h\,21^m\,35^s{\cdot}99$		
" " E.	$18^h\,20^m\,52^s{\cdot}54$	35 ·99	+ 43 ·45
	43 ·45		
	$18^h\,21^m\,35^s{\cdot}99$		
Greenwich	$18^h\,22^m\,35^s{\cdot}99$		

There is, moreover, a difference here of *one minute* in time, which, strange enough, occurs likewise in every one of the *ten further* observations of the transit of δ Ursæ Minoris made at Valentia, and which, more strangely still, has evidently escaped your attention. It would seem to require an explanation, which I must leave to you to give. The Greenwich observation is correct. But, setting that difference aside, the values "clock slow" always include the errors of observation, which are occasionally very considerable. On the same day *f. i.* at Valentia the "clock slow" is from an observation of α^2 Capricorni + 40ˢ·65; of λ Ursæ Minoris + 25ˢ·92. The value "used for comparison with chronometers" on that day is + 40ˢ·26; and though, by the adoption of a uniform "losing rate" of the clock, both the errors of the clock and of the observations, represented by the value of "clock slow," are eliminated to an extent probably coming up near to the truth: yet it is apparent that an element, "upon which," as you observe, "the difference of the longitude wholly depends," cannot be free from a *possible* error of two or three seconds in time at the very least.

Still less satisfactory is a glance over some of the columns of "chronometers' adopted losing rate;" and not only do the "resulting differences of longitude," concluded from the same chronometers, differ on different days up to two seconds in time and more; but even the "concluded longitudes," as obtained from different chronometers, vary for the distance

Greenwich—Liverpool,	between	12m	0s·82
	and	11	59·45
Liverpool—Kingstown,	"	12	32·21
	and	12	30·99
Kingstown—Valentia,	"	16m	53s·24
	and	16	51·27

When, therefore, in reference to the final results, namely,

Longitude of Liverpool west of Greenwich	12m	0s·05
" Kingstown " "	24	31·20
" Valentia " "	41	23·23

you state, that "the probable error of the last determination can scarcely amount to 0s·05 or 0s·06:" it appears to me, that such a statement is neither founded in reason, nor warranted by the facts of the case. Under any circumstances, the determination in question is affected by a positive error, arising from a neglect of principle, of double the value you name.

I have already remarked, that when sidereal time is used for the determination of longitude, the first condition in principle is, that the apparent relative positions of the Earth and the stars in space should absolutely be subject to no change. But in reality this condition is not fulfilled. There are two or three real motions, by the apparent effects of which the immediate result is modified. The first is the proper motion of the stars, describing vast orbits in space; but from their vaster distances this motion becomes perceptible to us only in the course of years, and, even if correctly known, might be neglected. The second motion is that of the Earth round the Sun. According to the present astronomical theory, the terrestrial orbit, with reference to the stars, is to be, and is, considered as a mere point; but erroneously so. If the Sun were stationary in space, the effect of the Earth's revolution about the central body of our system would be, to cause the poles of the heavens to describe apparent perfect circles about the poles of the ecliptic, and in which apparent motion all other stars participate; the form of the circle, described by the poles, assuming for the stars generally that of ellipses, the more elongated the nearer to the ecliptic, and the plane of the circles coinciding with the plane of the ecliptic, in the ecliptic itself. The radius of the circle of this apparent motion of the stars is 20″·5 in arc, nearly. But the Sun not being stationary in space, the latter motion is in appearance modified by, and combined with, the third motion, which is that of the Sun himself, accompanied by the entire train of luminaries which constitute the system, in a vast orbit through space; in a direction contrary to the direction in which the planets revolve round the Sun; and in a plane, inclined to the plane of the ecliptic at an angle of less than 2°. The effect of the latter motion is an apparent annual motion of the stars of 50″·1 in longitude from west to east; and *these apparent motions, combined, represent the phenomena, known in modern astronomy as those of the aberration of light and of the precession of the equinoxes, both at present erroneously explained and ascribed to purely imaginary causes.*

Now, you have, certainly, for the determination of clock-error used the apparent right ascensions of stars for the days, on which the observations were made; but this only shows a due application of the right principle to that particular part of the operation. It ought to have been applied as well to the final determination of the amplitude of the arc, and which has not been done; for, it is plain that, because the chronometers complete their twenty-four sidereal hours on the second of each two consecutive transits of the same star over the same meridian, that star has in the interval apparently moved, by its combined apparent motion, more or less, in the same direction in which the meridian moves. Hence, the meridian has taken a longer time to return to the star, than it would have done had the latter had no apparent motion; and hence the amplitude of the arc has been measured *too great* in the proportion of the united amount of the star's two apparent motions, and which, for the mean period of your operations—the middle of August, 1844—I find to have been very nearly $56''\cdot5$ in $360°$. The value of the corresponding correction to be applied to the arc, although inconsiderable, tends to produce a greater accordance between the chronometrical and the geodetic results of the undertaking. For, amounting to $0^s\cdot11$ for the chronometrical arc of $41^m\ 23^s\cdot23$ between Greenwich and Feagh Main, it reduces that arc to $41^m\ 23^s\cdot12$ and therefore the difference of $0^s\cdot16$ between it and the geodetic arc of........... $41\ \ 23\cdot07$ to .... $0^s\cdot05$

Very small as this difference is, a true theory may yet, perhaps, enable us to judge which of the two principal values is *probably* the correct one. For, if yours is the more accurate determination, the amplitudes of the previously calculated sections of the arc, as deduced from the geodetic elements, will give lengths of [illegible]s, differing to a greater extent from the corre[illegible]ng theoretical lengths, than those amplitudes w[illegible] when corrected in conformity with the chronomet[illegible]l result. For brevity's sake, I will apply this co[illegible]ection in the logarithmic form

$$41^m\ 23^s\cdot12 = 2483^s\cdot12 = \log.\ 3\cdot3949977$$
$$41\ \ 23\cdot07 = 2483\cdot07 = \ \ ,,\ \ 3\cdot3949790$$
$$\log.\ 0\cdot0000187$$

ubtractive, to the "measured length of a degree," as inally resulting from the geodetically computed ents.

Arc No. 75, middle latitude 52° 18′ 8″·3 :

Length of a degree, directly resulting from the given elements.* ... ... 34,993^{t}·3

Correction I. for difference of curvature, additive :

for lat. 55° 5′ 50″ ... ... l = 57128^{t}·6

L = 56726 ·3

402^{t}·3

log. 2·6045

$\frac{\pi}{180}$ = „ $\bar{2}$·2419

7^{t}·0 = log. 0·8464

Cos. 52° 18′ 8″ = „ 9·7864

log. 1·0600 = 11 ·5

35,004^{t}·8

35,004^{t}·8 = log. 4·5441277

Correction II. for theoretical error in the given "measured length of arc," subtractive : for lat. 52° 18′ 8″ by table, p. 203 ...log. 7·9939668

„ 7·9928775

„ 0·0010893

Measured length of a degree ... = 34,917^{t}·1 = log. 4·5430384

Computed length of a degree ... = 34,916 ·9

Difference... ... Geod. = + 0^{t}·2

Correction III. for assumed error in computed amplitude of arc, subtractive ... ... „ 0·0000187

Measured length of a degree ... = 34,915^{t}·6 = log. 4·5430197

Computed length of a degree ... = 34,916 ·9

Difference... ... Chron. = − 1^{t}·3

* For the type of calculation see page 183.

Arc No. 76, *middle latitude* 52° 14′ 51″·9 :

Length of a degree directly resulting from the given elements ... 35,035^{t}·5

Correction I. log. 0·8464
Cos. 52° 14′ 52″ „ 9·7869

log. 1·0595 = 11 ·4

35,046^{t}·9 = log. 4·5446498

Correction II. ... ... log. 7·9939706
„ 7·9928788 „ 0·0010918

Measured length of a degree ... = 34,958^{t}·9 = log. 4·5435580
Computed length of a degree ... = 34,959 ·6

Difference ... Geod. = − 0^{t}·7

Correction III. ... ... „ 0·0000187

Measured length of a degree ... = 34,957^{t}·4 = log. 4·5435393
Computed length of a degree ... = 34,959 ·6

Difference ... Chron. = − 2^{t}·2

Arc No. 77, *middle latitude*, 51° 11′ 24″·5 :

Length of a degree directly resulting from the given elements ... 35,863^{t}·8

Correction I. log. 0·8464
Cos. 51° 11′ 24″ „ 9·9971

log. 1·0493 = 11 ·2

35,875^{t}·0 = log. 4·5447919

Correction II. . . ... log. 7·9940491
„ 7·9929049 „ 0·0011442

Measured length of a degree ... = 35,780^{t}·6 = log. 4·5536477
Computed length of a degree .. = 35,780 ·3

Difference ... Geod. = + 0^{t}·3

Correction III.			log. 0·0000187
Measured length of a degree ...	= 35,779t·1	=	log. 4·5536290
Computed length of a degree ...	= 35,780 ·3		
Difference ... Chron. =	− 1t·2		

Arc. No. 78, *middle latitude* 50° 58′ 51″·5 :

Length of a degree directly resulting from the given elements ...		36,025t·9	
Correction I.	log. 0·8464		
Cos. 50° 58′ 51″ = „	9·7990		
	log. 1·0474	= 11·1	
		36,037t·0	= log. 4·5567486
Correction II. ...	... log. 7·9940644		
	„ 7·9929100		„ 0·0011544
Measured length of a degree ...		= 35,941t·3	= log. 4·5555942
Computed length of a degree ...		= 35,941 ·0	
Difference ... ... Geod.		= + 0t·3	
Correction III.			„ 0·0000187
Measured length of a degree ...		= 35,939t·8	= log. 4·5555755
Computed length of a degree ...		= 35,941 ·0	
Difference ... Chron.		= − 1t·2	

These are, indeed, remarkable and most satisfactory results; for it must be remembered, that the values of the corrections applied are deduced, upon strict mathematical principles, *from given facts.* Indeed, the chief correction is of a merely negative character; it simply undoes an error, into which General Colby had been led by Sir Isaac Newton's theory of universal gravitation, and more immediately by your own treatise on the figure of the Earth. The principle of the first correction need only be pointed out to be admitted. The precise value of the third correction alone may be questioned, because it is impossible for me in this place to establish it mathematically. The phenomena, however, on which it rests, though at present erroneously explained, are fully known. The quantity liable to any dispute, therefore, is very small. To increase the value itself would tend to approximate the chronometrical results still more closely to the geodetic ones; to diminish it would tend to increase the differences between them.

Admitting, then, the correction as it stands, the results arrived at, reflect the highest credit, independently of theory, both on the geodetic and on the chronometrical part of this extensive and interesting undertaking. From any triangulation, carried out under the late General Colby's direction, no other result could have been expected; whilst every one, who reads with but moderate attention your memoir upon the subject, must be impressed with the conviction, that nothing, which human foresight and human skill can accomplish, has been left undone, on your part, in order to insure the utmost possible accuracy of this great chronometrical experiment. It does equal honour to yourself, as its originator, and to the Lords Commissioners of the Admiralty, who so freely sanctioned and so effectually supported the proposed enterprise.

We have now, however, to compare the relative accuracy of the two processes. The differences, we have found, are as follows:—

	From purely Geodetic Elements.	From Geodetic Elements, corrected by the Chronometrical Result.
Arc No. 75	$+ 0^{s}\cdot 2$	$- 1^{s}\cdot 3$ in a degree.
" 76	$- 0\cdot 7$	$- 2\cdot 2$ "
" 77	$+ 0\cdot 3$	$- 1\cdot 2$ "
" 78	$+ 0\cdot 3$	$- 1\cdot 2$ "
Sums	4) $+ 0^{s}\cdot 1$	$- 5^{s}\cdot 9$
Mean differences	$= + 0^{s}\cdot 025$	$- 1^{s}\cdot 475$ in a degree.

These comparative differences would seem to speak for themselves. But it has to be remarked, that the normal measure, applied to the two different results, is deduced from the elements of the new theory of the Earth's figure. We cannot, therefore, as yet conclude with absolute certainty, that the geodetic results are the more accurate, and the chronometrical results the less accurate ones. Still, as those elements, I think, may already now be looked upon as firmly established, and will be further verified in a striking manner by the results of numerous pendulum experiments; and as, at all times we can but judge by our actual knowledge of the day: it appears to me *in the highest degree probable*, that the geodetically computed results of General Colby are the true results; that, consequently, the arc of longitude between Greenwich and Feagh Main measures 41^{m} 23^{s}·07 in time; and that, notwithstanding all the care, labour, and skill, devoted to the chronometrical experiment, its final result, independently of the theoretical error of — 0^{s}·11, is affected with a positive error of — 0^{s}·05 in addition to it.

And the more maturely we consider this interesting subject, the more we shall find our conclusion to gain in strength. You remark,* with reference to the "chains of geodetic lines:"—"Two sets of numbers in these tables enable us to form a judgment of the general accuracy of the geodetic process. One is the series of observed azimuths, as compared with the computed azimuths; the other is the series of observed latitudes, as compared with the computed latitudes." Now, the reverse of this is really the case. Of the two elements, the geodetical and the astronomical, the former is the more reliable one. "The geodetist," even von Zach, himself a distinguished astronomer, acknowledged,† "has to act the task-

* "Determination," p. 236.

† "Der Geodäte wird des Astronomen Zuchtmeister; der

master of the astronomer, and to correct him when he goes too far astray." All the elements, it has to be remembered, upon which the preceding calculations are based, and which have led to such highly satisfactory results, are the work of the geodetist, the result of geodetic measurement and triangulation. If, instead of the elements, furnished by General Colby, we were to use those, resulting from your chronometrical experiment and from the astronomically observed latitudes, discrepancies would appear, far more considerable than those, we have found; and the causes are, in a minor degree, the imperfections attaching to astronomical observation and to astronomical instruments; but in a far greater degree, *the errors and imperfections of the present astronomical theory*. I, therefore, consider it but a pleasing duty to quote your own words, in which you acknowledge General Colby's part of the labour. "I requested," you say,* "the assistance of Colonel (now Major-General) Colby for communication of the necessary elements for computing the chain of geodetic lines, connecting the Royal Observatory of Greenwich with the Liverpool Observatory, the temporary Kingstown Observatory, and the station

Erdmesser weist den Himmelsbeobachter zu rechte, wenn dieser sich gar zu grob verirrt."—*Monatl. Corresp.*, vol. xxviii. p. 140.

* "Determination," pp. 225, 226.

of Feagh Main. Colonel Colby complied with my request with a zeal and liberality which I had no title to expect. He not only took immediate steps for connecting, by a secondary triangulation, the Liverpool and Kingstown observatories with the great triangulation, but he also made many experimental calculations for determining the process which would be best employed in the computation, prepared the materials of every kind, effected the calculations throughout, and, finally, placed the whole at my service. In every part which required more detailed explanation, I received most powerful assistance from Captain Yolland, R.E., who, under Colonel Colby's instructions, superintended the principal part of these computations. The whole of this section may, therefore, be considered as the work of Colonel Colby, attached to the chronometrical work for the convenience of comparison of the two sets of results."

Taking, then, a general view of the entire operation, and considering it as the measurement of an arc of longitude, the undertaking has been carried out, *completely* and in the most admirable manner, by the late General Colby; while the chronometrical portion of it may be regarded as an important and ably conducted experiment for the verification of the results, arrived at by that distinguished geodetist.

This view does not in the slightest degree detract from the merits of your own share in the enterprise; but it places the latter in its *just* light, and on a true basis.

There is one more point connected with it, and bearing on the exact figure of the Earth, upon which I have again the misfortune to differ from you. It is this. From a comparison of, what you (erroneously) believe to be the true chronometrical and of the geodetic results of the measurement, you draw the conclusion,* that—"the verticals at Greenwich and Liverpool are less inclined to each other, or that the Earth's surface in England is flatter than accords with the geodetic calculation; that the inclination of the verticals at Liverpool and Kingstown is, sensibly, precisely the same as that given by the geodetic calculation, or that the Earth's surface in Ireland is more curved than the elements of the geodetic calculation imply." Such a conclusion implies the *absolute* truth of the results obtained by yourself, as well as the *absolute* truth of a large portion of the whole geodetic survey of Ireland. But, quite irrespective of the theoretical and positive errors, which I have shown these results to include, no one, in my opinion, can rationally or without a

* "Determination," p. 236.

bias, so strong as almost to amount to a fixed idea, assume results, subject to, and affected by, a number and variety of disturbing influences of considerable value, to be free from a POSSIBLE combined error of *less than half a second in time*, within the extent of an arc of ten degrees. Your conclusion, therefore, has no sufficient foundation.

This leaves me, finally, a somewhat ungrateful task to perform, namely, to call attention to the circumstance, that for many years past it has been the practice, to employ in the geodetic calculations, made in the Ordnance Map Office, the spheroidal elements resulting from your investigation of the figure of the Earth. The same elements have been used in the "Account of the Observations and Calculations of the Principal Triangulation, and of the Figure, Dimensions, and Mean Specific Gravity of the Earth, as derived therefrom; published, by order of the Master General and Board of Ordnance, under the direction of Lieutenant-Colonel James." *Hence, the whole of the results of the National Survey, so far as they are based upon those elements, are positively erroneous, and therefore valueless.*

The following table contains five sides of triangles, forming small arcs of parallels, included in the Indian meridian arc measured by Colonel Lambton. The longitudes, which have not been verified by any direct astronomical observations, were calculated to units of seconds only. On recalculating them, I have found one slight error. The longitude of Rungamalli, instead of 77° 58′ 22″, should have been 77° 58′ 23″, the more correct value being 77° 58′ 22″·7, which I have used, as well as the longitudes of 77° 56′ 34″·5 instead of 77° 56′ 34″·0 for Tirtapully, and of 77° 47′ 30″·5 instead of 77° 47′ 30″·0 for Paulamalli, the former being but very little in excess of the values found by computation. The lengths of degrees, which follow from the given elements, thus slightly corrected, as compared with the lengths calculated by *empirical* formulas, according to the present astronomical theory, and taken from the tables of Professor Encke, are:

	Arc No. 79.	Arc No. 80.	Arc No. 81.	Arc No. 82.	Arc No. 83.
"Measured"	55,014^{t}·8	55,532^{t}·6	55,810^{t}·0	55,999^{t}·0	56,247^{t}·1
Computed (Encke)..	55,144 ·4	55,658 ·6	55,941 ·0	56,128 ·3	56,379 ·3
Differences	− 129^{t}·6	− 126^{t}·0	− 131^{t}·0	− 129^{t}·3	− 132^{t}·2

Thus, a simple comparison between the "measured" and the calculated lengths of degrees of the parallel of about 12° of latitude shows, that the circumference of the Earth, in that latitude already, measures upwards of *fifty miles* less than the present theory supposes it to do, *even as computed by the aid of empirical formulas*,—or formulas, specially invented for the purpose of *adapting* the real longitudinal dimensions of our planet to its imagined shape. Are those "insignificant" discrepancies? Yet, the astronomer has but to revert to the theoretically and empirically determined lengths of *meridian* degrees in the same latitudes, as exhibited in the table of Indian arcs, pp. 176-177, and to multiply them with the cosine of the respective latitudes, in order to find, that the *true* measured lengths of our parallel degrees and their *true* differences, as compared with the computed lengths, must be approximatively:—

	Arc No. 79.	Arc No. 80.	Arc No. 81.	Arc No. 82.	Arc No. 83.
Meridian Degree "Measured"	56,766^{t}·0	56,756^{t}·0	56,750^{t}·0	56,747^{t}·0	56,742^{t}·0
Degree of the Parallel, "Measured"	54,801^{t}·0	55,306^{t}·0	55,582^{t}·0	55,767^{t}·0	56,011^{t}·0
Computed	55,144 ·0	55,659 ·0	55,941 ·0	56,128 ·0	56,379 ·0
Differences	− 343^{t}·0	− 353^{t}·0	− 359^{t}·0	− 361^{t}·0	− 368^{t}·0

But these differences, to which your particular attention has been called, closely as they already approach to the equatorial difference of 381^{t} in a degree, would seem to appear to you too "insignificant" still, *to deserve so much as a notice on your part.* I cannot, then, but regret my inability to arouse you to a sense of their importance; for, the astronomical error does *not*, in this instance, exceed the extent of one hundred and sixty-seven English miles—an "insignificant" portion of the terrestrial circumference, certainly, when the protean Earth is assumed to reach to the stars;* yet sufficiently large to involve the loss at sea of millions' worth of British property, and of the lives of thousands of British sailors.

It would have been somewhat difficult to discover the principle, on which the geographical longitudes and the measured lengths of the arcs of our table are calculated, had not Colonel Lambton, in the appendix to his first Memoir, supplied the necessary information.† It rests fundamentally on a combination of the length of an Indian meridian degree in latitude 10° 34′ 49″ with that, determined by Colonel Mudge for latitude 52° 2′ 20″; the ellipticity deduced being $\frac{1}{318 \cdot 13}$.

In conformity with this, the given measured lengths of arcs have been corrected and compared with the lengths, computed on the new theory. The sum of the resulting differences is found to be

$$\begin{array}{r} +\ 9^{t} \cdot 1 \\ -\ 4 \cdot 9 \\ \hline 5) = +\ 4^{t} \cdot 2, \end{array}$$

and therefore, the mean difference $= + 0^{t} \cdot 84$; exhibiting a greater accordance, than might have been expected from the differences of the meridian arcs.

* See pp. 66—67, 78.

† "Asiatic Researches," vol. xii., pp. 92, 93.

SECTIONS OF					
No.	Stations.	Latitudes.	Mean Latitude.	Differences in Latitude.	Longitudes.
		° ′ ″	° ′ ″	″	° ′ ″
79	Gootydroog ...	15 6 54·0	15 7 6·5	25·0	77 42 23·0
	Guddakulgooda	15 7 19·0			77 17 39·0
80	Savendroog ...	12 55 10·0	12 58 47·5	435·0	77 20 50·0
	Tirtapully......	13 2 25·0			77 56 34·5
81	Kumbetarine-malli	11 35 31·0	11 38 35·0	368·0	77 19 33·0
	Paulamalli......	11 41 39·0			77 47 30·5
82	Parteemalli ...	10 40 2·0	10 39 57·0	66·0	77 37 55·0
	Rungamalli ...	10 38 56·0			77 58 22·7
83	Perrioormalli..	9 12 21·0	9 12 30·0	18·0	77 33 0·0
	Meenachiporam	9 12 39·0			78 2 7·0

In addition to the preceding parallel sections of meridian degrees, I have computed a number of similar sides of triangles in various latitudes, and uniformly found the mean results, by their striking accordance with the corresponding values calculated on the new theory, to confirm the latter. As they would only serve, however, to multiply the evidence adduced, and which I consider amply sufficient for the purposes of this Letter, I omit them.

The arcs and sections of arcs of longitude, the measured lengths of a degree of which have here been compared with the corresponding theoretical lengths, represent an aggregate extent of 37° 0′ 14″·9 between 52° 18′ 57″ and 9° 12′ 30″ of north latitude.

INDIAN ARCS.						
No.	Amplitude of Arc.	Radius Vector.	Measured Length of Arc.	Length of a Degree.		Differences.
				Measured.	Computed.	
	′ ″	Toises.	English Feet.	Toises.	Toises.	Toises.
79	24 44·0	3,252,516	145,043·8	54,797·6	54,802·4	−4·8
80	35 44·5	3,251,912	216,038·9	55,309·3	55,306·1	+3·2
81	27 57·5	3,251,578	170,388·5	55,582·9	55,583·0	−0·1
82	20 27·7	3,251,356	122,303·1	55,769·0	55,767·3	+1·7
83	29 7·0	3,251,058	174,556·9	56,014·7	56,010·5	+4·2

The mean differences found between theory and measurement are:—

		In a degree.
I.	70 Sections of the great arc of the mean parallel ...	+ 0t·078
II.	4 Sections of the French arc across the mouth of the Rhone	+ 0 ·350
III.	4 Sections of the English arc between Greenwich and Valentia (Gen. Colby's determination) ...	+ 0 ·025
IV.	5 parallel Sections of Indian meridian arcs ...	+ 0 ·840

83 sections of arcs, the mean difference of all of which is + 0t·13, or *not quite ten English inches* in a degree.

The severest test which can be applied to any theory of the Earth's figure, are accurate measurements of degrees of longitude, because a slight difference in the mean latitude produces a considerable difference in the length of a degree. The above results would seem to indicate that the ellipticity of $\frac{1}{95}$, adopted by me at first, and the length of 56,726^{t}·3 for an equatorial-meridian degree, deduced from the results of the preliminary computation both of measured arcs and of pendulum experiments, represent so closely the actual proportions and dimensions of the Earth, as to require, for the present at least, no modification.

XVII.

While the geodetic measurements of meridian and parallel degrees may be regarded as a mechanical process of determining the linear dimensions of the Earth, the determination of its proportions, on the principles of gravity, is effected by pendulum experiments.* The former operation gives us the linear

* "The quantity to be measured," you remark ("Figure of the Earth", p. 220), "is the velocity, which gravity creates in any freely descending body, by its action continued during one second (or any other given duration) of time. This will be known if we determine the space, through which gravity draws the body in that time . . . With the pendulum we can ascertain this (in the opinion of some philosophers) within a four-hundredth-thousandth part of the whole. This accuracy arises partly from the circumstance, that the experiments with the pendulum may be continued for a very long time, with the certainty that there is no interruption between the

measure of the circumferences of great circles, formed by the terrestrial surface; the latter gives us a scale of the radii of these circles. Hence, we have but to apply to the unit of our scale the corresponding linear value, in order to determine, by the pendulum also, the linear dimensions of our globe; and now that the theory of this most valuable instrument has been placed on a secure basis, it can no longer be said that it "gives no information respecting the magnitude of the Earth." * Indeed, as the length of the seconds-pendulum and the law of its variation are known, it is plain that the linear extent of the terrestrial radii is derivable from it even in a *direct* manner, though with no sufficient degree of accuracy to render the method useful.

The following table comprises the results of the most reliable pendulum experiments, which have thus far been made, and among which the extensive series of observations by General Sabine holds the first place.

end of one vibration and the beginning of the next, and partly from the very remarkable fact, that the friction and other disturbing causes, which ultimately put a stop to the experiment, do not injure its accuracy so long as it lasts." We shall presently find, that this view as to the degree of accuracy, obtainable by pendulum experiments, is not borne out by empirical results. The true theory of pendulum motion has been indicated at pp. 131-133.

* Galloway, "Figure of the Earth," p. 572.

No.	Observer.	Station.	Latitude.	Radius Vector.
			North.	
			° ′ ″	Toises.
1	Sabine	Spitzbergen	79 49 58	3,283,332
2	Sabine	Melville Island......	74 47 0	3,282,044
3	Sabine	Greenland	74 32 19	3,281,970
4	Foster	Port Bowen	73 13 39	3,281,555
5	Sabine	Hammerfest	70 40 5	3,280,657
6	Sabine	Hare Island	70 26 0	3,280,570
7	Sabine	Drontheim	63 25 54	3,277,573
8	Kater	Unst	60 45 28	3,276,257
9	Sabine	Brassa	60 1 0	3,275,877
10	Lütke	St. Petersburg	59 56 31	3,275,838
11	Svanberg	Stockholm............	59 20 31	3,275,526
12	Kater	Portsoy	57 40 59	3,274,641
13	Lütke	Sitka	57 2 58	3,274,305
14	Kater	Fort Leith	55 58 41	3,273,715
15	Bessel	Königsberg	54 42 50	3,273,013
16	Sabine	Altona	53 32 45	3,272,359
17	Kater	Clifton	53 27 43	3,272,304
18	Lütke	Petropaulowski......	53 0 53	3,272,049
19	Kater	Arbury Hill	52 12 55	3,271,588
20	Sabine	London	51 31 8	3,271,185
21	Sabine	London	51 31 8	3,271,185
22	Fallows	London	51 31 8	3,271,185
23	Foster	London	51 31 8	3,271,185
24	Kater	London	51 31 8	3,271,185
25	Goldingham...	London	51 31 8	3,271,185
26	Hall............	London	51 31 8	3,271,185
27	Brisbane	London	51 31 8	3,271,185
28	Foster	London	51 31 17	3,271,186
29	Sabine	Greenwich......... ...	51 28 40	3,271,161
30	Sabine	Greenwich.	51 28 40	3,271,161
31	Foster	Greenwich............	51 28 40	3,271,161
32	Foster	Greenwich............	51 28 40	3,271,161
33	Lütke	Greenwich............	51 28 40	3,271,161
34	Biot	Dunkirk	51 2 12	3,270,904
35	Kater	Shanklin Farm......	50 37 24	3,270,662
36	Biot	Paris	48 50 14	3,269,612
37	Borda	Paris	48 50 14	3,269,612
38	Sabine	Paris	48 50 14	3,269,612

No.	Length of Seconds-Pendulum.		Difference.	Number of Vibrations in a Mean Solar Day.		Difference.
	Observed.	Computed.		Observed.	Computed.	
	Inches.	Inches.				
1	39·21469	39·21199	+·00270	86483·28	86480·15	+3·13
2	·20700	·20429	+·00271	....	471·66	....
3	·20335	·20384	—·00049	470·72	471·17	—0·45
4	·20419	·20137	+·00282	470·48	468·44	+2·04
5	·19475	·19601	—·00126	461·14	462·53	—1·39
6	·19840	·19548	+·00292	....	461·95	....
7	·17456	·17758	—·00302	438·64	442·20	—3·56
8	·17162	·16971	+·00191	435·40	433·53	+1·87
9	·16930	·16744	+·00186	....	431·00	....
10	....	·16721	....	432·20	430·76	+1·44
11	·16541	·16534	+·00007	....	428·69	....
12	·16159	·16005	+·00154	424·70	422·86	+1·84
13	....	·15805	....	420·54	420·65	—0·11
14	·15546	·15452	+·00094	418·02	416·99	+1·03
15	·15072	·15031	+·00041	....	412·11	....
16	....	·14640	....	408·98	407·80	+1·18
17	·14600	·14607	—·00007	407·48	407·22	+0·26
18	....	·14455	....	408·90	405·76	+3·14
19	·14250	·14171	+·00079	403·63	402·71	+0·92
20	....	....	....	399·72	400·06	—0·34
21	....	....	....	400·00	400·06	—0·06
22	....	....	....	399·76	400·06	—0·30
23	....	....	....	399·90	400·06	—0·16
24	·13929	·13938	—·00009	400·00	400·06	—0·06
25	....	....	....	400·00	400·06	—0·06
26	....	....	....	400·00	400·06	—0·06
27	....	....	....	400·00	400·06	—0·06
28	....	....	....	400·00	400·06	—0·06
29	·13983	·13924	+·00059	400·67	399·90	+0·77
30	....	....	....	400·72	399·90	+0·82
31	....	....	....	398·90	399·90	—1·00
32	....	....	....	399·46	399·90	—0·44
33	....	....	....	399·24	399·90	—0·66
34	·13773	·13770	+·00003	....	398·19	....
35	·13614	·13625	—·00011	396·40	396·60	—0·20
36	·12851	·12997	—·00146	....	....	....
37	39·12770	39·12997	—·00227	....	....	....
38	....	....	....	86388·30	86389·67	—1·37

No.	Observer.	Station.	Latitude.	Radius Vector.
			North.	
			° ′ ″	Toises.
39	Freycinet......	Paris	48 50 14	3,269,612
40	Duperrey......	Paris	48 50 14	3,269,612
41	Biot	Clermont	45 46 48	3,267,794
42	Biot	Milan	45 28 1	3,267,606
43	Biot	Padua	45 24 3	3,267,568
44	Biot	Fiume	45 19 0	3,267,518
45	Biot	Bordeaux	44 50 26	3,267,233
46	Biot	Figeac	44 36 45	3,267,097
47	Duperrey......	Toulon	43 7 20	3,266,208
48	Biot	Barcelona	41 23 15	3,265,177
49	Sabine	New York	40 42 43	3,264,777
50	Biot	Formentera	38 39 56	3,263,575
51	Biot	Lipari	38 28 37	3,263,465
52	Lütke	Bonin Island.........	27 4 12	3,257,291
53	Hall............	St. Blas...............	21 32 24	3,254,810
54	Freycinet	Mowi..................	20 52 7	3,254,539
55	Sabine	Jamaica...............	17 56 7	3,253,436
56	Campbell	Jamaica........	17 56 7	3,253,436
57	Freycinet......	Guam	13 27 51	3,252,042
58	Lütke	Guam	13 26 21	3,252,035
59	Goldingham...	Madras	13 4 9	3,251,936
60	Sabine	Trinidad	10 38 56	3,251,352
61	Foster	Trinidad	10 38 56	3,251,352
62	Foster	Porto Bello	9 32 30	3,251,122
63	Sabine	Sierra Leone.........	8 29 28	3,250,927
64	Lütke	Ualan	5 21 16	3,250,477
65	Hall..	Galapagos	0 32 19	3,250,181
66	Sabine	St. Thomas	0 24 41	3,250,179
67	Goldingham...	Pulo Guansah Lout	0 1 49	3,250,177
			South.	
68	Freycinet......	Rawak	0 1 34	3,250,177
69	Foster	Para	1 27 0	3,250,199
70	Sabine	Maranham............	2 31 35	3,250,244
71	Foster	Maranham...........	2 31 43	3,250,244
72	Foster	Ascension	7 55 23	3,250,831
73	Sabine	Ascension	7 55 48	3,250,832
74	Duperrey......	Ascension	7 55 48	3,250,832

No.	Length of Seconds-Pendulum.		Difference.	Number of Vibrations in a Mean Solar Day.		Difference.
	Observed.	Computed.		Observed.	Computed.	
	Inches.	Inches.				
39	....	....	....	86388·01	86389·67	— 1·66
40	....	....	....	388·56	389·67	— 1·11
41	39·11615	39·11909	—·00294	....	377·66	....
42	·11603	·11796	—·00193	....	376·44	....
43	·11896	·11773	+·00123	....	376·16	....
44	·11788	·11743	+·00045	....	375·83	....
45	·11296	·11573	—·00277	....	373·94	....
46	·11215	·11491	—·00276	....	373·04	....
47	·10952	·10959	—·00007	367·16	367·17	— 0·01
48	·10432	·10342	+·00090	....	360·36	....
49	·10120	·10103	+·00017	358·06	357·72	+ 0·34
50	·09510	·09382	+·00132	....	349·76	....
51	·09828	·09317	+·00511	....	349·03	....
52	....	·05617	....	322·06	308·16	+13·90
53	·03829	·04130	—·00301	288·80	291·72	— 2·92
54	·04690	·03967	+·00737	297·52	289·94	+ 7·58
55	·03503	·03306	+·00197	284·66	282·62	+ 2·04
56	·03220	·03306	—·00086	....	....	....
57	·03379	·02468	+·00911	282·98	273·36	+ 9·62
58	....	·02464	....	280·64	273·32	+ 7·32
59	·02630	·02405	+·00225	272·36	272·66	— 0·30
60	·01888	·02055	—·00167	266·78	268·80	— 2·02
61	....	....	....	267·24	268·80	— 1·56
62	....	·01917	....	272·01	267·27	+ 4·74
63	·01997	·01800	+·00197	267·54	265·98	+ 1·56
64	....	·01530	....	275·44	263·00	+12·44
65	·01717	·01353	+·00364	264·56	261·02	+ 3·54
66	·02074	·01351	+·00723	268·84	261·00	+ 7·84
67	·02126	·01350	+·00776	266·64	261·00	+ 5·64
68	·01433	·01350	+·00083	261·46	261·00	+ 0·46
69	....	·01363	....	260·61	261·15	— 0·54
70	·01213	·01391	—·00177	259·19	261·45	— 2·26
71	....	....	....	258·74	261·45	— 2·71
72	....	·01742	....	272·26	265·34	+ 6·92
73	39·02363	39·01747	+·00620	272·56	265·35	+ 7·21
74	....	....	....	86272·06	86265·35	+ 6·71

No.	Observer.	Station.	Latitude.	Radius Vector.
			South.	
			° ′ ″	Toises.
75	Sabine	Bahia..................	12 59 21	3,251,916
76	Lütke	St. Helena............	15 54 59	3,252,763
77	Foster	St. Helena............	15 56 7	3,252,768
78	Legentil	Madagascar	17 40 0	3,253,343
79	Duperrey......	Mauritius	20 9 23	3,254,259
80	Freycinet......	Mauritius	20 9 56	3,254,263
81	Lacaille	Mauritius	20 10 0	3,254,263
82	Freycinet......	Rio de Janeiro	22 55 13	3,255,389
83	Hall............	Rio de Janeiro	22 55 22	3,255,390
84	Lütke	Valparaiso............	33 2 30	3,260,386
85	Brisbane	Paramatta............	33 48 43	3,260,810
86	Freycinet......	Port Jackson.........	33 51 34	3,260,836
87	Duperrey......	Port Jackson.........	33 51 40	3,260,837
88	Foster	Cape of Good Hope	33 54 37	3,260,862
89	Freycinet......	Cape of Good Hope	33 55 15	3,260,870
90	Fallows	Cape of Good Hope	33 55 56	3,260,877
91	Foster	Monte Video	34 54 26	3,261,429
92	Duperrey......	Falkland Island ...	51 31 44	3,271,190
93	Freycinet......	Falkland Island ...	51 35 18	3,271,225
94	Foster	Staten Island	54 46 23	3,273,046
95	Foster	Cape Horn	55 51 20	3,273,650
96	Foster	South Shetland......	62 56 11	3,277,336

We have here before us the results of 55 observations of the seconds-pendulum, and of 76 observations of the invariable pendulum: in all 131 experiments; which number, however, includes 8 of the former, and 15 of the latter kind, differing to a remarkable extent, as compared with the results generally, from the computed values. General Sabine observes of these discrepancies, that "they are due, in a far greater degree, to local peculiarities, than to what

No.	Length of Seconds-Pendulum.		Difference.	Number of Vibrations in a Mean Solar Day.		Difference.
	Observed.	Computed.		Observed.	Computed.	
	Inches.	Inches.				
75	39·02433	39·02393	+·00040	86272·38	86272·53	– 0·15
76	....	·02902	....	288·29	278·15	+10·14
77	....	·02905	....	288·29	278·19	+10·10
78	·02670	·03250	–·00580	....	282·00	....
79	....	·03799	....	297·60	288·06	+ 9·54
80	·04684	·03801	+·00883	298·08	288·09	+ 9·99
81	·03890	·03801	+·00089	....	....	....
82	·04350	0·4476	–·00126	293·48	295·56	– 2·08
83	....	·04482	....	294·90	295·64	– 0·74
84	....	·07472	....	328·16	328·66	– 0·50
85	·07452	·07726	–·00274	331·48	331·47	+ 0·01
86	....	·07742	....	334·06	331·63	+ 2·43
87	·07919	·07743	+·00176	332·94	331·65	+ 1·29
88	....	·07757	....	331·33	331·81	– 0·48
89	·07800	·07762	+·00038	331·58	331·87	– 0·29
90	....	·07766	....	332·56	331·91	+ 0·65
91	....	·08097	....	334·36	335·57	– 1·21
92	....	·13941	....	399·84	400·09	– 0·25
93	39·13781	·13962	–·00181	396·74	400·32	– 3·58
94	....	·15051	....	415·22	412·33	+ 2·89
95	....	·15414	....	417·98	416·33	+ 1·65
96	....	39·17616	....	86444·52	86440·63	+ 3·89

may be more strictly called errors of observation;"* and already Mr. Baily† had expressed the opinion, "that the vibrations of a pendulum are powerfully affected, in many places, by the local attraction of the substratum on which it is swung, or by some other direct influence at present unknown to us, and the effect of which far exceeds the errors of observation."

* "On the Ellipticity of the Earth," Cosmos, vol. iv., p. 479.

† "Memoirs of the Royal Astronomical Society," vol. vii.

Without entering here into a discussion of the probable cause of these apparent anomalies, it is obvious that they have no relation to the Earth's figure, and, therefore, should not be considered in connection with it. The only difficulty consists in fixing the value, which is to impart to a given difference the character of an anomaly, as distinguished from a possible error of observation. According to General Sabine, the *probable* error in the observed number of vibrations of a single station is $1^s{\cdot}7$. I have extended this limit to $4^s{\cdot}0$, and the similar value of 0·00040 inch. in the length of the seconds-pendulum; rejecting only, for theoretical comparison, the differences in excess of these quantities, as arising from particular "influences at present unknown to us." A similar process has been adopted by General Sabine in his interesting paper on the Laws of the Phenomena of the larger Disturbances of Magnetic Declination.*

The remaining number of normal experiments, the observed results of which have been compared with the results computed, according to the new pendulum laws, on the new theory of the Earth's figure, comprises 47 observations of the seconds-pendulum, and 61 observations of the invariable pendulum,—in all

* "Proceedings of the Royal Society," vol. x., p. 624.

108 observations. On adding the differences of the former, we find the sum to be

inch.
+ 0·03745
− 0·03236

47) + 0·00509 inch.; and, therefore, the mean difference = + 0·0011 inch., being somewhat more than *the thousandth part of a line.* The sum of the latter differences is

vibrations.
+ 39·19
− 34·71

61) + 4·48 vibr.; and, therefore, the mean difference = + 0·074 vibr., being rather more than *the seven-hundredth part* of a second.*

* Comparing your own differences (Figure of the Earth, p. 230), as calculated by the aid of *empirical* formulas, with the differences resulting from the new theory, we find:

No.	Length of Seconds-Pendulum. Differences.		Number of Vibrations. Differences.	
	Empirical Formulas.	New Theory.	Empirical Formulas.	New Theory.
1	+ ·00388	+ ·00270	+ 4·3	+ 3·1
2	+ ·00380	+ ·00271	......	......
3	+ ·00055	+ ·00049	+ 0·6	+ 0·5
4	+ ·00381	+ ·00282	+ 4·2	+ 2·0
5	− ·00034	− ·00126	− 0·4	− 1·39
6	+ ·00380	+ ·00292	......	......
7	− ·00243	− ·00302	− 2·7	− 3·56
8	+ ·00240	+ ·00191	+ 2·7	+ 1·87
Etc.				
	+ 0·1824 − 0·0277	+ 0·1336 − 0·0477	+ 11·8 − 3·1	+ 7·04 − 5·40
Mean Difference	8) + 0·1547 + 0·0193	8) + 0·0859 + 0·0107	6) + 8·7 + 1·45	6) + 1·64 + 0·27

Separating the experiments, made in each hemisphere, we have,—

	Seconds-Pendulum.	Invariable Pendulum.
	Differences.	Differences.
Northern Hemisphere,	38 obs. } inch. + 0·03319 — 0·02478	41 obs. } vibr. + 25·92 — 19·92
	+ 0·00841	+ 6·00
	Mean = inch. + 0·00022	Mean = vibr. + 0.146
Southern Hemisphere,	9 obs. } + 0·00426 — 0·00758	20 obs. } + 13·27 — 14·79
	— 0·00332	— 1·52
	Mean = inch. — 0·00037	Mean = vibr. — 0·076

Nor will it be uninteresting to compare the individual results of the principal observers. We find them to be :—

Observers.	Seconds-Pendulum.		Invariable Pendulum.	
	Number of Observ.	Mean Differences.	Number of Observ.	Mean Differences.
		inch.		vibr.
Sabine............	14	+ 0·00051	16	— 0·11
Foster............	...	....	12	+ 0·19
Kater	7	+ 0·00070	7	+ 0·71
Hall	2	+ 0·00031	4	— 0·05
Biot	11	— 0·00026	...	...
Freycinet	4	— 0·00047	6	— 0·79
Duperrey	2	+ 0·00084	4	— 0·02
Lütke	...	....	5	+ 0·66

I have previously remarked, that the linear dimensions of the Earth might now be determined by the pendulum also. This implies that, approximatively at least, the latitude is to be found by it as well. Indeed, all we require to know for either purpose is the true linear extent of the terrestrial radius vector to any given point of the Earth's surface, and which we obtain from observed lengths of the seconds-pendulum by the formula

$$p^2 - \frac{p^2}{R} = r,$$

and from the observed number of vibrations of the invariable pendulum by the formula

$$s^4 - \frac{s^4}{R} = r;$$

upon which, computing the latitude from the table given at pp. 140-2, we proceed as usual.

The following table exhibits the latitudes, and the lengths of degrees of the meridian and of the parallel, thus deduced from ten of the best observations of the seconds-pendulum and an equal number of observations of the invariable pendulum, made by different observers, in all parts of the world, and in both hemispheres, as compared with the observed latitudes and the lengths of the corresponding degrees of latitude and longitude, computed on the new theory of the Earth's figure.

No.	Stations.	Observed Elements.		Radius Vector.		Differences.
		Length of Seconds Pendulum.	Number of Vibrations.	Deduced from Pendulum Observation.	Computed from Theory.	
		Engl. Inch.		Toises.	Toises.	Toises.
11	Stockholm	39·16541	....	3,275,538	3,275,527	+ 11
13	Sitka	....	86420·54	3,274,351	3,274,305	+ 46
15	Königsberg......	39·15072	....	3,273,081	3,273,013	+ 68
17	Clifton..........	39·14600	....	3,272,292	3,272,301	− 9
91	Falkland Island ..	....	86399·84	3,271,153	3,271,191	− 38
28	London	....	86400·00	3,271,177	3,271,187	− 10
24	London	39·13929	....	3,271,170	3,271.185	− 15
34	Dunkirk	39·13773	....	3,270,910	3,270,904	+ 6
35	Shanklin	....	86396·40	3,270,632	3,270,662	− 30
44	Fiume	39·11788	....	3,267,593	3,267,518	+ 75
47	Toulon..........	39·10952	....	3,266,195	3,266,208	− 13
47	Toulon..........	....	86367·16	3,266,206	3,266,208	− 2
49	New York	39·10120	....	3,264,806	3,264,776	+ 30
49	Yew York	....	86358·06	3,264,830	3,264,776	+ 54
89	Cape of Good Hope	39·07800	....	3,260,932	3,260,870	+ 62
89	Cape of Good Hope	....	86331·58	3,260,829	3,260,870	− 41
85	Paramatta	....	86331·48	3,260,812	3,260,810	+ 2
59	Madras..........	....	86272·36	3,251,891	3,251,936	− 45
75	Bahia	39·02433	....	3,251,982	3,251,917	+ 65
75	Bahia	....	86272·38	3,251,894	3,251,917	− 23

These results are sufficiently satisfactory, and not, I venture to think, devoid of interest. It must be remembered, that the object of this Letter is not that of a treatise on the Earth's figure, and that the table of *radii vectores* at pp. 140-2, was not intended to be applied to the computation of latitudes from pendulum elements. Yet, the differences, which our table exhibits, will fairly bear a comparison with the results of direct geodetic measurements, and in many cases are greatly exceeded by the differences, which attend the latter; while

Latitude.			Length of a Degree of the Meridian.			Length of a Degree of the Parallel.		
Deduced from Pendulum Observation.	Observed.	Differences.	Deduced from Pendulum Observation.	Computed from Theory.	Difference.	Deduced from Pendulum Observation.	Computed from Theory.	Differences.
° ′ ″	° ′ ″	′ ″	Toises.	Toises.	Toises.	Toises.	Toises.	Toises.
59 21 50	59 20 31	+ 1 19	57,168·9	57,168·7	+0·2	29,132·4	29,151·1	−18·7
57 8 3	57 2 58	+ 5 5	57,148·2	57,147·4	+0·8	31,011·2	31,083·3	−72·1
54 50 4	54 42 50	+ 7 14	57,126·0	57,124·9	+1·1	32,901·2	32,998·8	−97·6
53 26 26	53 27 43	− 1 17	57,112·3	57,112·6	−0·3	34,019·3	34,002·4	+16·9
51 27 50	51 31 44	− 3 54	57,092·4	57,093·0	−0·6	35,569·0	35,518·7	+50·3
51 30 19	51 31 17	− 0 58	57,092·8	57,093·0	−0·2	35,537·0	35,524·5	+16.5
51 30 25	51 31 8	− 0 43	57,092·7	57,093·0	−0·3	35,535·6	35,526·5	+ 9·1
51 2 44	51 2 12	+ 0 32	57,088·2	57,088·1	+0·1	35,891·5	35,898·3	− 6·8
50 34 18	50 37 24	− 3 6	57,083·3	57,084·2	−0·9	36,254·3	36,215·1	+39·2
45 26 35	45 19 0	+ 7 35	57,030·3	57,029·0	+1·3	40,013·4	40,102·1	−88·7
43 6 3	43 7 20	− 1 17	57,005·9	57,006·1	−0·2	41,623·0	41,608·6	+14·4
43 7 9	43 7 20	− 0 11	57,006·1	57,006·1	0·0	41,610·6	41,608·6	+ 2·0
40 45 43	40 42 43	+ 3 0	56,981·6	56,981·1	+0·5	43,159·5	43,191·6	−32·1
40 48 15	40 42 43	+ 5 32	56,982·0	56,981·1	+0·9	43,132·4	43,191·6	−59·2
34 1 56	33 55 15	+ 6 41	56,914·0	56,912·9	+1·1	47,165·9	47,226·9	−61·0
33 50 46	33 55 15	− 4 29	56,212·2	56,912·9	−0·7	47,267·7	47,226·9	+40·8
33 48 54	33 48 43	+ 0 11	56,911·9	56,911·9	0·0	47,284·6	47,286·3	− 1·7
12 53 56	13 4 9	−10 13	56,756·2	56,757·0	−0·8	55,324·0	55,286·8	+37·2
13 14 33	12 59 21	+15 12	56,757·8	56,756·7	+1·1	55,248·6	55,304·4	−55·8
12 54 39	12 59 21	− 4 42	56,756·3	56,756·7	−0·4	55,321·4	55,304·4	+17·0

the *maximum* of difference in the lengths of meridian degrees, as calculated from accurate pendulum observations and compared with the corresponding theoretical values, is only 1ᵗ·3.

I need hardly say, that the "lengths of parallel degrees, deduced from pendulum observations," are computed with the "latitudes deduced from pendulum observations" also; but that the corresponding lengths "computed from theory," are calculated with the "latitudes observed." It is from this, that the differences chiefly arise. Were the results of pendulum experiments combined with astronomically observed latitudes, *the differences for degrees of parallels would be no greater, than they are for meridian degrees.* The importance of the pendulum, as an instrument for determining not only the proportions of the true figure of the Earth, but *its dimensions as well,* is thus rendered apparent.

General Sabine remarks*: — "The discordances, which were formerly found in the results of pendulum experiments, when the number of stations were comparatively few, or when the arc, which they included, was of small dimension, appear to have produced in M. de Humboldt's mind an impression, that the pendulum is a less likely means of obtaining a well-assured conclusion respecting the figure of our planet, than the measurement of degrees. It is not surprising, indeed, when we look back upon the formidable array of distinguished geometricians and astronomers, who have taken part, for more than a century past, in the measurement of degrees, that there should be a bias in favor of the ultimate conclusions from a method, on which such a prodigious amount of time, means, and skilled labour have been expended. Still, there have not been wanting eminent persons, who have been of a

* "On the Ellipticity of the Earth," p. 481.

different opinion, and who, viewing the discrepancies, which have also presented themselves in the ellipticity, deduced from different measured arcs—discrepancies arising from causes similar to those, which affect pendulum results—have anticipated, that the pendulum would eventually be found amongst the most accurate means of determining the general configuration of the globe." I fully concur with General Sabine in this anticipation. Two things, however, are necessary for its realization: to ascertain, by a series of extensive and systematic experiments with pendulums of different substances, firstly, the exact empirical values of the equatorial elements of this instrument; and, secondly, either the cause or the laws of the anomalies of its action, so as to enable us, if not to remove the anomalies themselves, to compute their amount.

"The differences of opinion," it is observed by General Sabine, "which once prevailed regarding the causes of such discrepancies have now passed away; and they are now universally admitted, I believe, to be the effects of the unequal density of the superficial strata of the Earth." Perfectly reconcilable with my own theoretical views of the pendulum and of gravity, as this hypothesis is, I must still, with every deference to General Sabine's opinion, be permitted to consider it subject to some doubt. I will here call attention only to two facts. General Sabine himself relates:—"Captain Foster was furnished with two invariable pendulums of precisely the same form and construction as those, which had been employed by Captain Kater and myself. Both pendulums were vibrated at all the stations; but, from some cause, which Mr. Baily was unable to explain, *the observations with* ONE *of them were so discordant at South Shetland, as to require their rejection.*" The second fact is told by the observation No. 81, as compared with the observations No. 79 and No. 80 of our table. Without placing any undue reliance on the result obtained by Lacaille, yet, more especially in connection with the circumstance just alluded to, it appears to me of a nature to justify us in suspending our judgment as to the real causes of the anomalies in question.

Moreover, I may state positively, that these causes are unconnected with the discrepancies in the results of measurements of degrees. The latter spring from a source, which I cannot here explain.

XVIII.

The whole of the preceding conclusions are based on an assumption of modern astronomy, adopted also by me; namely, that the equatorial circumference of the Earth, as that of a spheroid of revolution, forms a circle. And this renders it necessary that I should say a few words on the essay, lately published by General von Schubert,* and in which he arrives at the conclusion, that the equator is not of a circular, but, like the meridian circumference of the Earth, of an elliptical shape; so that the general appearance of either hemisphere would somewhat resemble the form of a plain oval dish-cover. The direction of the minor equatorial axis, according to General von Schubert, is long. 148° 44′ and 328° 44′, that of the major axis long. 58° 44′ and 238° 44′ from Ferro; and the two rates of polar depression, which thus result from his analysis, he finds to be $\frac{1}{302}$ for the least, and $\frac{1}{292}$ for the greatest, meridian.

It need hardly be said, that the facts and arguments, which have been urged against the common theory, apply with double force to the hypothesis of General von Schubert. Moreover, we find, by arranging our results of pendulum observations according to the geographical longitudes of their different stations, that they betray not so much as the slightest indication in favor of General von Schubert's analytical deductions. The same is found to

* See p. 1.

be the case, on the "differences" of the seventy sections of the great arc of the mean parallel being placed in a similar order, when they exhibit a uniformity even more remarkable than appears from a simple inspection of their latitudinal succession. And lastly, the origin of those deductions is readily explained. Modern astronomy, as has been proved, takes the equatorial circumference of the Earth to be nearly 170 miles in excess of what it really is. A true analytical calculus, therefore, having to dispose that linear excess of circumference about a *given* axis—the equatorial diameter of the Earth—would, in order to effect this, have to convert the assumed circular form of the terrestrial equator into that of an ellipse, described about the equatorial diameter of the Earth as its minor axis. Hence the result arrived at by General von Schubert, and which merely proves, in accordance with his own impression, that he had introduced into his calculation of the Earth's figure a somewhat less erroneous method, than astronomy and analysis had made use of before him.

The hypothesis of General von Schubert therefore, notwithstanding the importance attached to it by yourself and other astronomers, is devoid of every real foundation, and, under any circumstances, leaves the results, obtained by me, perfectly intact.

These results, summed up, show the following differences between theory and observation :—

44 meridian arcs, representing a distance in arc of $80^\circ\ 57'\ 23''\cdot8$	Mean Difference. + $0^{t}\cdot11$ in a degree.
83 sections of arcs of parallels, representing a distance in arc of $37^\circ\ 0'\ 14''\cdot9$	Mean Difference. + $0^{t}\cdot13$ in a degree.
108 observations of the pendulum, included within a meridional distance in arc of $142^\circ\ 46'\ 9''$	Mean Differences. + 0·00011 inch in the length of the seconds pendulum. + 0·074 vibr. in the number of vibrations of the invariable pendulum in a mean solar day.

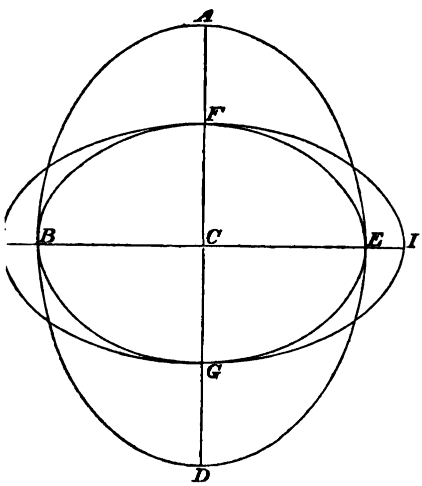
I may, therefore, be permitted to say, that the new theory of the figure of the Earth *realizes, at its very birth, what physical astronomy and analysis, by their united efforts of a century and a half, have vainly endeavoured to accomplish ;* and that, henceforth, the polar elongation of the Earth must be considered an indubitable cosmical fact.

XIX.

Having established, I venture to think, by conclusive proofs and arguments, the propositions concerning the true shape and dimensions of the Earth, stated at the commencement of this letter, I have still, somewhat more fully, to point out their important bearing upon science, on the one hand ; upon commerce and navigation, on the other.

It will be readily understood, from the mere fact of the whole of that mass of complicated error, which constitutes the present astronomical doctrine of the Earth's true figure, having literally sprung from Sir Isaac Newton's theory of gravitation as its source, and analysis having imprinted on it the stamp of "mathematical truth," that the result, here arrived at, must tend to shake the entire system of modern theoretical and physical astronomy to its very foundations. It does more: for, not only has theoretical astronomy, on the strength of the law of universal gravitation, shown the polar flattening of the Earth to be a physical necessity, as the higher calculus has mathematically "proved" it to be a physical fact; but from this presumed fact, again, the consistency of the Newtonian theory and of the entire system of modern astronomy with the actual phenomena of the Cosmos has been demonstrated, in reference to a series of numerous and important features. Thus, those (supposed) phenomena of motion, the expression of which is comprehended by the term *perturbations*, constitute a vital (though purely imaginary) element in the present system of theoretical and physical astronomy; and, to a great extent, the whole theory of perturbations rests on the presumed fact of the Earth's polar depression. That the Moon, *f. i.*, is found, at a given moment, in its actual place, and not in a very different position, is, according to modern astronomy, solely owing to the circumstance, that our Earth bears a protuberance of exactly the presumed magnitude, and precisely in the presumed place. To the same physical cause, according to astronomy, is to be attributed the phenomenon, commonly termed "the precession of the equinoxes," which in reality is the expression of the motion of our entire solar system, in a vast orbit, through space; but in which modern astronomy recognizes only a very slow retrogradation of the poles of the terrestrial axis of rotation about the center of the Earth—a kind of *tumbling about*, arising from the Earth's presumed shape, *i.e.*, its equatorial protuberance of $\frac{1}{299}$. Nay, analysis is led to vaunt herself, that by her formulas, the astronomer, without leaving his observatory, is enabled to read, from certain (imaginary) anomalies in the motion of the heavenly bodies, produced by the same (imaginary) equatorial protuberance, *the Earth's interior condition;* and which (imagined) interior condition of the Earth in its turn, is constituted into one of the fundamenta elements, made to support the system of modern theoretical and physical astronomy — one of those highly artificial and extremely complicated structures, which the pride of human imagination and misguided intellect delights to contemplate, but which crumble into dust at the first breath of truth from a newly conquered, though ever so humble a *cosmical fact.*

Such a fact, at the time when the Ptolemean system was believed to be as firmly and as durably established as the Newtonian system is now, was the Earth's daily rotation about itself, on being rediscovered by Copernic; such a fact, if I mistake not, is the polar elongation of the Earth, to which I have here ventured to call your attention.



A B D E—the Earth's real shape.

F B G E—first shape of the theoretical Earth.

F H G I—second shape of the theoretical Earth.

Fig. 28.

The practical consequences to navigation, which must result from the astronomical error of taking the equatorial circumference of the Earth at nearly 170 miles in excess of its real dimensions, are obvious. In the annexed diagram, Fig. 28, the real figure of the Earth, and the shapes of the two earths of the theory of modern astronomy, are represented. From *F B G E*, the least erroneous of these two imaginary shapes, the linear dimensions of meridian degrees are, by means of empirical formulas, deduced with tolerable accuracy. It is from the second imaginary shape, with its equatorial diameter in excess, and its polar diameter in deficiency, of the truth, that the linear dimensions of degrees of the parallels of latitude are computed; and it is the excess of these computed values over the actual distances, which involves so fearful a loss of life and property at sea: because each nautical mile, which really measures only 945·388 toises, is *reckoned* to measure 951·809 toises.*

For the purpose of illustration, we will suppose the simple case of a steamer, pursuing her course due west from a point *a* in the equator, Fig. 29. Let it be assumed that in *a* a series of reliable astronomical observations were taken, which determined the ship's position to be lat. 0° 0′ 0″ and long. 100° 0′ 0″ west of Greenwich; that subsequently the weather continued calm and fair, but clouded,

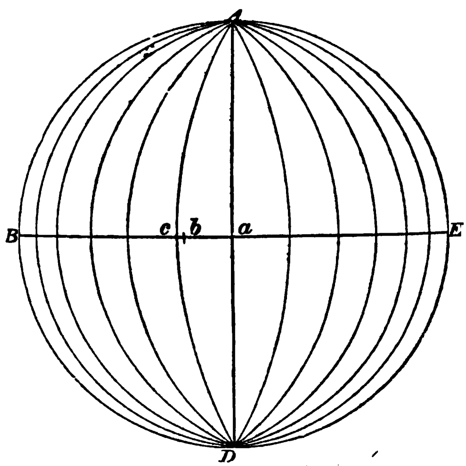


FIG. 29.

* The French toise is = 6·3946 English feet, nearly.

preventing further astronomical observations from being made; that the steamer's log is well and carefully kept; and that in lat. 0° 0′ 0″ and long. 112° 0′ 0″ west of Greenwich some sunken rock or other point of danger, *c*, to be avoided, is known to lie. Now, let it be further assumed, that at some time on the third day after the astronomical observations were taken at *a*, and when the vessel is known to near the point *c*, she, in the position *b*, is found, by reckoning, to have made 85,800 knots, or 715 nautical miles, equal, by the actual measure of the log-line, to 680,543 toises, a knot being equal to $\frac{951\cdot809}{120}$, or nearly 7·9317 toises. The distance between *a* and *c* is known to be exactly 12°, or 720 nautical miles. Consequently, at *b*, the vessel is found, and believed to be still, at a distance of five nautical miles from *c*, the point of danger; and her course due west is continued. But hardly a few seconds elapse, and she strikes upon the sunken rock; becoming, perhaps, a total wreck, with the loss of her entire cargo and every soul on board. Because, though at *b* she had sailed, by the log-line, the distance of 715 nautical miles only, these 715 nautical miles were measured at the rate of 951·809 toises each; whereas, if they had been measured at the *true* rate of 945·388 toises each, the whole distance sailed, namely, 680,543 toises, would have been found $= \frac{680543}{945\cdot388}$ or 719·85, nautical miles. Thus, when, in consequence of an erroneous astronomical theory, the vessel, at *b*, was believed to be still 5 nautical miles distant from *c*, and therefore proceeded on her course, that distance was actually only 20 knots; and when the steamer had no apparent cause to apprehend any danger for her safety, she was in the very act of foundering.

It may, at first sight, appear somewhat puzzling to the general reader, that the number of nautical miles between *a* and *c* is, according to both theories, identical, viz., 720; at the same time, that I make out a difference between them of five nautical miles. The explanation, however, will appear as simple, when it is remembered that the nautical mile is an angular, rather than a linear measure, being one of $360 \times 60' = 21{,}600$ equal parts of the Earth's equatorial circumference, whatever be the true linear value of that circumference. Hence, considered as a linear measure, it has as yet no definite value, and its correctness depends absolutely on the correct linear measurement of an equatorial degree. If, therefore, the circumference of the Earth is taken too great by 166 or 167 miles, the nautical mile, being one of its equal parts, and the subdivisions of the nautical mile or knots of the log-line—by which the distance sailed by a vessel is actually measured—are likewise taken too great; and, consequently, the linear distance sailed by a vessel, when reduced to angular distance, is reduced by means of too great a unit of measure; whence the number of nautical miles, sailed both by computation and by the log-line, falls short of the true number.

From the diagram, Fig. 30, this will appear more plain; *c‴ c b a* representing an equatorial arc of the Earth's actual surface, of which the arc *c a* subtends an angle of 12°. It measures 720 nautical miles, and

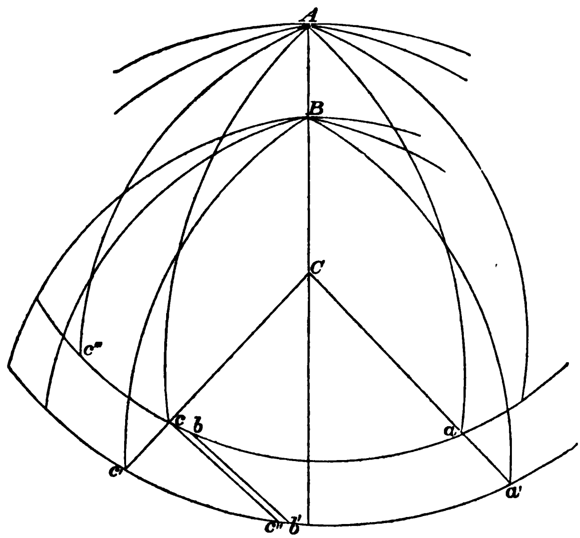


FIG. 30.

A C—the true polar radius.
B C—the theoretical polar radius.
c a—an arc of 12° on the true surface of the Earth.
c′ a′—an arc of 12° on the theoretical surface of the Earth.

824·41 common English miles. *c′ c″ a′* is the corresponding arc of the Earth's imaginary surface, according to the teaching of modern astronomy, likewise subtending an angle of 12° and measuring 720 nautical miles, but 829·96 common English miles. Hence, when by actual distance, sailed on the Earth's actual surface, the vessel has advanced from *a* to *b*, *i.e.*, 719·85 (true) nautical = 824·2 common English miles, she is, by actual measurement, and according to the astronomical theory, computed and believed to have advanced from a' to b' (a' b' being taken = a b), *i.e.*, 715 (erroneous) nautical = 824·2 common English miles. But the sunken rock is known to lie in lat. 0° 0′ 0″ and long. 112° 0′ 0″, determined by the terrestrial radius C c'; consequently, on the imaginary surface of the Earth in c', on its real surface in *c*. Therefore, when the vessel is actually in *b*, she is believed to be in b'; and when she is found to be still distant from the point of danger, by the arc c' b' on the imaginary surface, = the arc c''' b on the real surface of the Earth, measuring 5·7 common English miles, she is only distant from it by the arc b c on the real surface = the arc c'' b' on the imaginary surface of the Earth, measuring 0·15, or only $\frac{1}{6}$ of a nautical mile; *i.e.*, she is on the very point of striking.*

* In a flippant notice of the first edition of this "Letter," the *Athenæum*, which, so far as science is concerned, would appear to seize upon every opportunity of stultifying itself, in all seriousness

Let it not be thought, that accidents of this nature are but of rare occurrence. It is only when they

propounds, in its number for October 23rd, 1861, the following absurdity :—"If the wrong method of *laying down places on the earth* be accompanied by a corresponding *wrong* method of going from place *to* place, the *result* is that *the way* from place to place *is known.*" Hence, "*for aught we* [the *Athenæum*] *can see* in the tract, no such consequences would be incurred as the author describes, even if he were right." It then proceeds to illustrate its ignorance by misrepresenting and generalizing an exceptional case, thus :—"Many remember that, in the long Indian voyage, when *no land whatever* had been seen *since the coast of England was lost*, the captain would announce the approaching appearance of the *flagstaff* at Madras to his weary passengers, who would see it *within an hour or two* of his prediction, *in the direction which he had given.*" Whereupon that "scientific" journal continues :—"This *by no means* proves that our author is *wrong ;* all it *proves*, is that *Madras* and the way of looking out for it *were made equally wrong ;* which will do. It would be *better* that both were right ; but *it seems* that, practically, there is no *astronomical danger* in our actual navigation."

The *Athenæum* has fallen into two palpable errors. In the first place, it concludes, because the captains of certain vessels bound from England for Madras, on nearing that port, happened to be out of reckoning, "within an hour or two," *on the safe side*, and "practically" *did* reach their destination, that therefore *an erroneous theory is of no moment whatsoever to navigation*, since it is "practically" attended with no danger. But will the *Athenæum* explain how, for instance, Her Majesty's ship *Conqueror* came to be lost? How so many other noble vessels, perfectly found, perfectly manned, *perfectly navigated*, have been wrecked *in calm weather*, not only in a dark night, or in a fog, but in broad daylight and sunshine—in the former case upon the coasts, in the latter upon sunken rocks—FROM "BEING OUT OF RECKONING," under circumstances *which*—till now—*have baffled every satisfactory explanation of that fact?*

The second error the *Athenæum* shares with its defunct associate happen under very unusual circumstances and in remarkably fine weather, that they attract particular

in ignorance, the *Literary Gazette*, which, in its number for October 19, 1861, states:—"Every existing chart *and sea-route* having been elaborated under the assumption of this [the Newtonian] hypothesis, *it follows that the locality of rocks, shoals, and other maritime dangers has all along been misplaced in greater or less degree.*" What the late *Literary Gazette* "misplaces" on the charts, the *Athenæum*, as we have seen, "misplaces" *on the Earth itself*. Both labour under the same mistake, in considering it "obvious that, be Mercator exact or not in projecting nominally the unseen peril at a certain degree *or* minute in latitude *or* longitude, the navigator, *guided by the same calculations, will be equally well advertised of its proximity.*" They rely on the "practical method." Well, then, there is a very simple "practical method" for these "self-betrayed illusionists," to "force a conviction of their illusions upon them"-selves. Let them, on some open hilly ground, the surface of which we will assume to represent that of a section of a sphere of 1,500 paces—say of two feet each—in circumference, choose a starting-point; let there, at a certain linear distance from this point, a bar be placed across the hill, at a foot or two from the ground; let them determine the *angular* distance of the bar from the starting-point, with reference to the center of the assumed sphere, at 5°, and *quite correctly so;* let them, on the strength of some old tradition, take the circumference of our sphere to measure 1,800 paces, at two feet each, and hence *correctly calculate* the linear distance between the starting-point and the bar to be 25 paces. Let them now, blindfolded, make straight for the bar. They naturally will count their paces, expecting to reach the bar at the 25th pace. But, though again they count *correctly*, and take *correct* paces—at two feet each,—already at the 21st pace *they will tumble over the bar*, and thus "practically" convince themselves—if the first result should not satisfy them, let them repeat the experiment—that there is a difference between 25 and 20; and, consequently, that attention; and then, appearing altogether inexplicable, they are attributed either to the sunken rock being erroneously laid down in the charts, *i.e.*, to its geographical position being erroneously determined, or to temporary under-currents and similarly imaginary causes; whereas the true cause, never dreamt of, is an erroneous astronomical theory. What a fearful share an error of 167 miles in the linear dimensions of the Earth's equatorial circumference, measuring only 24,732 miles, *i.e.* an error of 1: 148 (nearly $\frac{7}{10}$ per cent.), in connection with mists, fogs, currents, storms, and other elements of disaster at sea, must have in the annual losses, in life and property, which arise from this cause to commerce and navigation, and which have now reached a colossal amount, it is not difficult to realize or to estimate.

These losses have, of late years, attracted more and

there *is* danger in Navigation, on the strength of her faith in Sir Isaac Newton's erroneous *theory*, taking the equatorial circumference of the Earth *at one hundred and sixty-six miles greater than it is.*

I need hardly point out to the rational and attentive reader, that the whole of my very argument is based *on the correctness of the charts.* Those of the English Admiralty are probably, in every respect, *as accurate and perfect as an unbounded liberality, the most unremitting zeal, and an unexampled solicitude for the safety of the British navy and the lives of our seamen can render them.*

But the more perfect the charts and the more perfect the navigation, *the greater the danger arising from the present astronomical theory.*

more the attention and the solicitude of both the Governments of England and of France. The tonnage of the French shipping, during the last 22 years, has increased from 1,072,968 tons to 6,693,000 tons; that of the shipping of England, during the short period between 1840 and 1857, from 9,440,000 tons to 23,180,000 tons. Mr. Lissignol, from whose lately published essay, "*Les Accidents de Mer*," * I borrow this notice, truly observes in reference to it:—"The danger of shipwrecks and casualties at sea is, therefore, no longer the exclusive concern of a small number of individuals, who, of their own free will, expose themselves to that danger: it is a thing threatening to the community. Henceforth, the necessity of keeping maritime disasters within the smallest possible bounds will have to be considered as something more than a simple question of speculative philanthropy; because it has become a question of public interest, concerning the commonwealth."

From the Parliamentary "*Returns of Wrecks and Casualties which occurred on and near the Coasts of the United Kingdom*" (1853—1860),† I collect the following summary:—

* Paris, 1860, p. 8.

† The returns for the past year, 1861, still unpublished, will have to record a heavy increase of wrecks and casualties, as compared with the returns for 1860.

Years.	Number of Wrecks and Casualties. British Ships.	Foreign Ships.	Total Ships.	Tons burthen of Ships.	Approximate value of Cargoes.	Ships.
1852	946	169	1,115			
1853	688	144	832	no returns.		
1854	824	163	987			
1855	974	167	1,141	———		
1856	916	237	1,153	229,936	no returns.	no returns.
1857	930	213	1,143	218,570		
1858	961	209	1,170	205,243	———	———
1859	1,228	188	1,416	222,926	£893,000	£870,000
1860	1,186	186	1,372	215,000	no returns.	no returns.
Total	8,653	1,676	10,329	1,091,675	£1,763,000	
An. av.	961	186	1,148	218,335		

Years.	No. of hands employed in the above Ships.	Number of Lives Imperilled.	Saved.	Lost.	Rate per cent. of Lives Lost.	Saved.
1852				920		
1853	no returns.	no returns.	no returns.	689	no returns.	no returns.
854		———	———	1549	———	———
.855	———	1,884	1,388	469	26·3	73·7
1856	10,014	2,137*	2,243	521	24·4	75·6
1857	9,819	2,200	1,668	532	24·2	75·8
1858	8,979	1,895	1,555	340	18·0	82·0
1859	10,538	3,977	2,332	1,645	41·4	58·6
1860	9,816	2,688	2,152	536	19·9	81·1
Total	49,166	14,781	11,338	7,201	25·7	74·3
An. av.	9,833	2,465	1,890	800		

* This number is exclusive of 627 persons, taken off the *Racer* without risk.

The total value of the annual losses of the French mercantile navy, Mr. Lissignol* computes at 31 millions of francs; the number of total wrecks at 425 vessels of the aggregate burthen of 29,325 tons;† being to the number of casualties of all kinds in the approximate proportion of 1 : 3.‡ The whole sea-going mercantile navy of France at the close of 1858, he states to have consisted of 15,175 vessels of the collective burthen of 1,052,535 tons.§

Now, if we would gain a tolerably correct idea of the annnal losses at sea, sustained by the commercial navies of civilized nations, whose nautical science and handbooks of navigation are based on the theory of gravitation and the doctrines of modern astronomy, the principal elements to be taken into account are, the aggregate tonnage of those navies; the proportion of the tonnage of vessels, to which casualties happen, to that aggregate tonnage; and, again, the proportion of losses, in life and property, to such casualties.

I possess, to my regret, no statistical information of the total shipping of civilized nations at the present time; but, as that of England and France alone represents a burthen exceeding 30,000,000 tons, if we estimate the collective tonnage of all the navies of England, France, Russia, Prussia, Hanover, the Hanse Towns, Holland, Belgium, Denmark, Norway,

* "Les Accidents de Mer," p. 142.
† Ibid., p. 130.
‡ Ibid., p. 136.
§ Ibid., p. 141.

Sweden, Spain, Portugal, Italy, Austria, Greece, Turkey, British India, Australia, the Canadas, the United States, the Brazils, the Republics of Central America, &c., at 40,000,000 tons, the estimate must be considered a very low one. I would underrate rather than exaggerate. From Mr. Lissignol's data, out of 15,175 French vessels of 1,052,535 tons burthen, there met, in 1858, with accidents of various kinds 425 vessels of 29,325 tons burthen, totally lost, and approximately double their number, or 850 vessels of 56,650 tons burthen, more or less damaged: making a total of 1,275 vessels of nearly 88,000 tons burthen. This would give a proportion of about 8 per cent. Mr. Lissignol computes the proportion to be, for the merchants' navy of France about 7 per cent., for that of England about 6 per cent.; and he remarks,* that out of 25,115 vessels, registered in the United kingdom, there having, in 1856 [884, should be] 916 British ships met with casualties on the English coasts: this would give a proportion of 3½ per cent., *from wrecks and casualties on the coasts of the United Kingdom alone.* If, then, we take this proportion for the totality of wrecks and casualties, which occur to the mercantile navies of civilized nations, at 6 per cent., we may estimate the aggre-

* "Les Accidents de Mer," p. 45.

gate tonnage of vessels (belonging to these navies) which meet with accidents at sea, at the very least, in round numbers, at 2,400,000 tons. But, from our table of Parliamentary Returns we find, that the proportion of actual annual losses, in life and property, may be taken at 800 lives to 218,335 tons, and at £1,763,000 worth of property to 222,926 tons; which would show the losses, arising out of disasters at sea to the commerce and navigation of civilized nations, to amount to no less an *annual* total than 8,794 lives and £18,980,000—or nearly twenty millions pounds sterling worth of property.*

Now, if we estimate the share in these losses, due to the present erroneous astronomical theory, at only from 5 to 6 per cent.—which makes it in proportion to the total tonnage in question only between 1 : 400 and 1 : 300, whereas the proportion of the error of 167 miles, committed by astronomers in determining the linear dimensions of the Earth's circumference, is to that circumference of 24,732 miles as 1 : 148,—we find the losses at sea, ***resulting from Sir Isaac Newton's theory of gravitation*** and the present system of astro-

* An almost identical value is obtained from the French data, furnished by Mr. Lissignol. Let it be remembered that it is a sum which, in the short space of 40 years, would pay off the whole national debt; and that nearly one-half of it falls to the share of England.

nomy, as applied to the practical purposes of navigation, to amount, in round numbers, to at least *five hundred human lives, and a million pounds sterling worth of property, annually;* and startling as such a loss, owing to such a cause, may appear, it is yet, in all probability, estimated greatly below the truth.

In an ordinary case of scientific inquiry, the conclusive manner, in which the Earth's polar elongation has been here proved, would call for no further confirmation of its manifest truth. But the magnitude of the combined interests, material and scientific, involved in the solution of this problem, render it, in my opinion, imperative, that it should be verified by direct measurement; and that, consequently, an expedition for this, in combination with more general purposes, should be sent out to various parts of the equator. With the present extended resources at the command of science, the time, labour, and expense required for a great undertaking of this kind might, without the least detriment to its results, be reduced to comparatively moderate proportions, by connecting with it—I speak advisedly—a plan of maritime triangulation which, by the aid of properly adapted instruments, I not only hold practical, but which, so far as equatorial arcs of longitude are in question, I am inclined to consider practical even by itself. That I am fully aware of the attending difficulties, as well as of the insufficiency of the methods and means of observation at present employed at sea, I need hardly remark.

Considering, then, that Commerce and Navigation, under the blessing of God, are the mainsprings of England's greatness, power, and prosperity, and that the Royal Observatory of Greenwich has been expressly founded to serve their purposes; and looking back upon the succession of illustrious men, who have adorned the position which you now hold, and who to their eminent merits and attainments united in so high a degree the love of science and of truth: I ventured, in the first edition of this Letter, to express the earnest hope that, in your capacity of Astronomer Royal, you might see occasion to recommend to the favorable consideration of Her Majesty's Government the expediency of such an expedition: a hope, however, which has not been realized. Deeply in the interest and for the credit of science I regret this; but confidently, too, I rely on the power and ascendency of truth—

Magna est veritas et prævalebit.

The wisdom of Her Majesty's Government, the learning of this country, and the good sense of the people of England will not fail, with the full merits of the case laid before them, to take the right view of a question, on which not only depend the future of astronomy and the advancement of human knowledge, but which involves, moreover, to so vast an extent the interests of British commerce and navigation.

Permit me to add that, personally, I am willing to place the most charitable construction upon your motives, and to be reminded by Sir David Brewster that—"Men are not necessarily obstinate because they cleave to deeply-rooted errors; nor are they absolutely dull, when they are long in understanding and slow in embracing newly-discovered truths."

I have the honour to be, with profound respect,

Sir,

Your most humble and obedient servant,

JOHANNES VON GUMPACH.

Guernsey, *March*, 1862.

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